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The ADVANCEMENT OF SCIENCE

Vol. XII, No. 48

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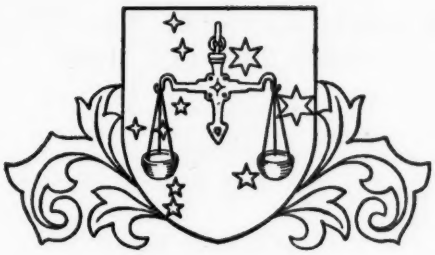
1831  1956

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CLIMATIC FLUCTUATIONS IN BRISTOL[1]

by

DR. F. G. HANNELL

IT IS probably true to say that very few regions have such variable weather as the British Isles, and this results from the fact that we experience marked fluctuations between a continental climate on the one hand and a maritime climate on the other. The highest mean summer [2] temperature recorded at Bristol since 1880 (65·0° F. in 1899) was more than 0·5° F. above the normal figure for Santiago on the north-west coast of Spain, whereas the lowest (57·3° F. in 1922) was within 0·5° F. of the mean for Hernosand on the east coast of Sweden. Similarly, the highest mean winter [2] temperature experienced at Bristol during the past 75 years (44·8° F. in 1899) was 0·5° F. above that normally recorded at Marseilles, whereas the lowest (34·8° F. in 1895) was within 0·5° F. of the mean for Vestmannaeyjar off the south coast of Iceland. Thus the extreme range in both summer and winter mean temperatures at Bristol since 1880 has been equivalent to a change of latitude of no less than 20°.

This paper constitutes an attempt to analyse Bristol's meteorological records over the past 75–100 years with the object of delimiting such climatic fluctuations as have been experienced here, and then to offer a possible explanation of their occurrence. At the outset, however, an explanation must be given of what is implied in the use of the words 'continental' and 'maritime' phases, and this can best be presented by reference to maps of mean atmospheric pressure in January and July (Fig. 1). On each of these the 30-inch isobar has been emphasised, for this in general forms the boundary between the main high-pressure and low-pressure systems.

Fundamental to the thesis which follows is a recognition of the fact that stations in Southern England lie very close to this boundary both in winter and summer, and that a small-scale migration of the boundary to the north or south would suffice to bring about very marked changes in seasonal climates. If it moves to the north in winter, Southern England will come under the influence of cold air from the Continent. Temperatures will then be unusually low, and, since depression tracks are deflected northwards, the winter rainfall total will be notably less than is recorded at other times. This constitutes what will later be referred to as a 'continental winter.' On the other hand, if the 30-inch isobaric boundary moves south in winter, numerous depressions will cross Southern England bringing heavy rain, and temperatures will be mild consequent upon the importation of latent heat from the comparatively warm Atlantic. Such conditions are typical of what is to be described as a 'maritime winter.' If the Azores high-pressure system extends further to the north than is usual in summer, the tongue which covers Spain and France will also embrace Southern England. Skies here will then be comparatively cloudless and temperatures high. Depression tracks will be deflected even further to the north than is usual in this season, with the result that the total rainfall in such a 'continental summer' will be small. On the other hand, if the Azores high-pressure system is displaced southwards, the summer skies will be cloudy and the temperatures lower. Moreover, Southern England's rainfall total in such a 'maritime summer' will be high, due to the fact that depressions will cross the country well to the south of their normal summer routes.

[1] Address delivered to Section E (Geography) on September 1, 1955, at the Bristol meeting of the British Association.

[2] The primary seasons are designated as follows: Summer, June to August; Winter, December to February, and whenever a seasonal figure is quoted this represents the mean derived from recordings in the three relevant months.

Thus during a continental phase winters will be cold and dry and summers hot and dry. Therefore the difference between summer and winter temperatures will be at a maximum, and annual rainfall totals will be less than those normally recorded. During a maritime phase winters will be mild and wet and summers cool and wet. The difference between summer and winter temperatures will now be at a minimum, and annual rainfall totals will be unusually high.

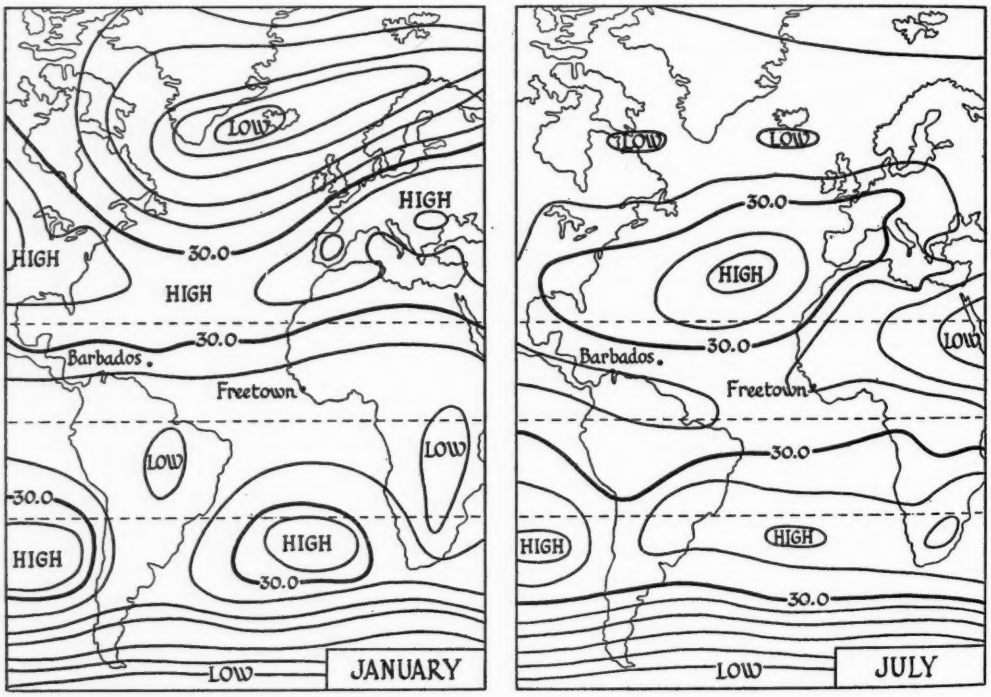


FIGURE 1

Mean atmospheric pressure in tenths of an inch

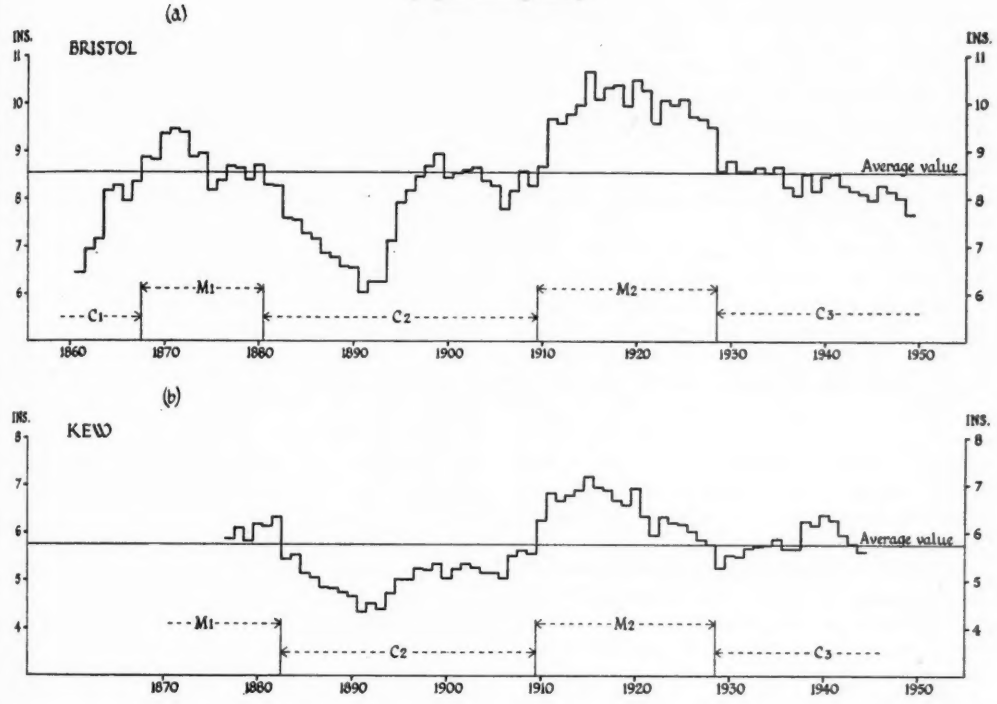
So far as temperatures are concerned, this paper is based upon daily readings taken at Clifton College from 1880 to 1927, and at the University of Bristol's Agricultural and Horticultural Research Station at Long Ashton from 1920 onwards. The 7-year overlap between these two records makes it possible to compute what would appear to be a very satisfactory set of correlation corrections, and these, when applied to the Clifton College records, result in the compilation of a reasonably homogeneous set of readings covering the 75-year period between 1880 and 1954.

With regard to rainfall, the Bristol Waterworks Company has kindly made available a completely homogeneous record obtained from a rain-gauge which is mounted on the roof of its headquarters near the city's centre. Readings there commenced in 1855, and thus the end of 1954 saw the completion of a 100-year record. A detailed examination of the data shows that over the past 35 years annual totals from this elevated rain-gauge have been about 9 per cent. below those from the ground-level rain-gauge at Long Ashton. However, as this paper is concerned only with rainfall fluctuations and not with absolute amounts, the 100-year record of the Bristol Waterworks Company has been used as the basis of all rainfall arguments.

It is now proposed to deal first with such rainfall fluctuations as have been experienced in Bristol during the past 100 years. Conclusions derived from the

WINTER RAINFALL

(11-year running means)



SUMMER RAINFALL

(11-year running means)

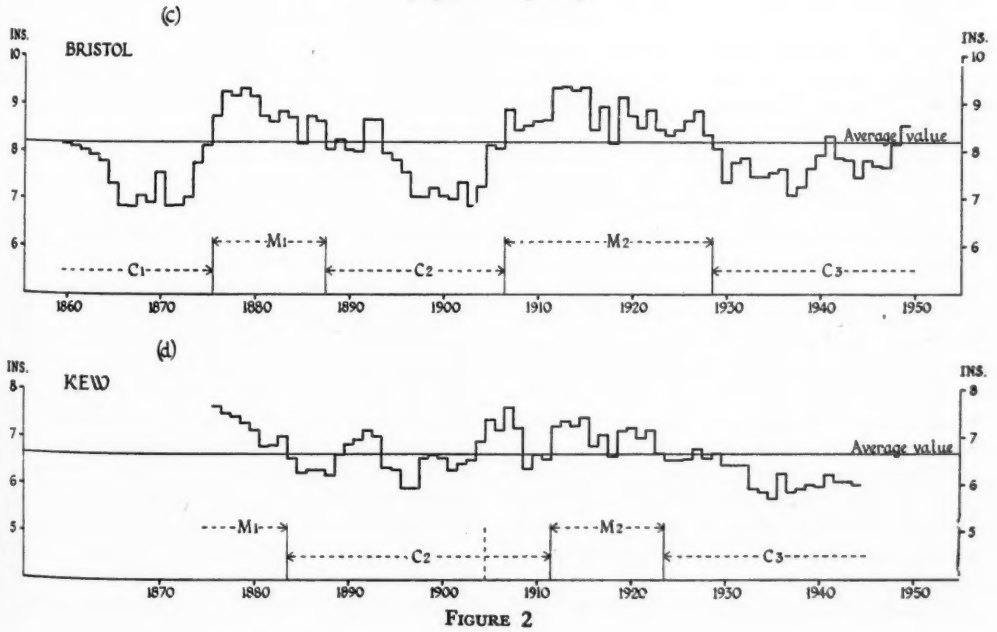


FIGURE 2

Kew figures (1) will also be presented, not only to show that similar fluctuations have been experienced on the eastern side of Southern England, as might well be expected, but also to demonstrate that such fluctuations as are revealed by the Bristol record are not statistical accidents.

Fig. 2*a* shows 11-year running means of winter rainfall at Bristol, and Fig. 2*b* those at Kew over a somewhat shorter period. As is well known, each value shown represents the mean winter rainfall over an 11-year interval stretching 5 years on either side of the plotted position. The horizontal line running across each of the graphs represents the average value of all the 11-year running means for the post-1870 period ; that is, the period covered by the somewhat shorter Kew record. Phases characterised by values lying below this long-term average are those during which continental influences prevailed (C_1, C_2 and C_3), whilst others marked by values lying above the average are those during which maritime influences were dominant (M_1 and M_2). The record for Kew shows a precisely similar phase pattern as that which is derived from the Bristol figures, and it is particularly worthy of note that although the change from M_1 to C_2 is delayed by 2 years in the case of Kew, the two succeeding phase changes occur at exactly the same points on both graphs.

On the graph which shows 11-year running means of summer rainfall at Bristol (Fig. 2*c*) the change from one phase to the next is very clearly defined, and the phase pattern is the same as that indicated on the corresponding graph of winter rainfall. The Kew graph (Fig. 2*d*) is much less regular, particularly during the second continental phase, which at first sight would seem to embrace two very brief maritime interludes. However, this lack of regularity is due not to maritime incursions, but to the fact that during a hot continental summer the London area often experiences heavy thundery rain. A detailed examination of the rainfall figures for individual summers reveals that it was this factor which accounted for the minor humps in the Kew graph centred on the 1891 and 1906 plots, and that the M_2 phase began with the 1912 plot and not with that for 1904 as might at first seem to have been the case. So far as summer rainfall is concerned, a comparison of the relevant graphs shows that a maritime phase at Kew begins 5 years later than at Bristol and ends some 4 or 5 years earlier. This is a reflection of the fact that the Kew summers are normally more continental in character than those of Bristol. Thus, during the early and late years of a maritime phase, when the oceanic influences are relatively weak they fail to penetrate as far east as Kew. There a maritime phase in summer rainfall is experienced only when the oceanic influences are operating at full strength.

The 11-year running mean graphs of annual rainfall at Bristol and at Kew (Figs. 3*a* and 3*b*) display a remarkable degree of similarity. The phase patterns are identical, and the junctions between M_1 and C_2 and between C_2 and M_2 occur within a year of each other on both. On the winter and summer graphs for Bristol (Figs. 2*a* and 2*c*) the change from M_2 to C_3 occurs within a year of its position on the annual rainfall graph for Kew. On the annual graph for Bristol, however, this point is delayed by 6 years owing to a succession of very wet autumns between 1924 and 1932 inclusive.

Although such graphs of running means have been widely used in the study of climatic fluctuations, they are in fact subject to three serious disadvantages (2) :

(i) Since each of the values plotted is the mean for a number of years, in this case 11, the graphs obscure the precise date of each phase change.

(ii) They tend to be misleading on account of the fact that a single exceptionally dry or wet year has a marked effect on successive values equal in number to the years covered by the running mean period. For example, Fig. 3*a* shows a distinct hollowing between the 1916 and 1926 plots which is entirely due to the fact that the annual rainfall total at Bristol in 1921 was exceptionally low. In the compilation of this graph the 1921 figure was first used in computing the 1916 plot, and was last used in deducing that for 1926, the years between which the hollowing occurs. On the other hand, the rainfall total at Bristol in 1924 was the heaviest on record. This gives rise to a marked reduction in the degree of hollowing after 1919, for it was

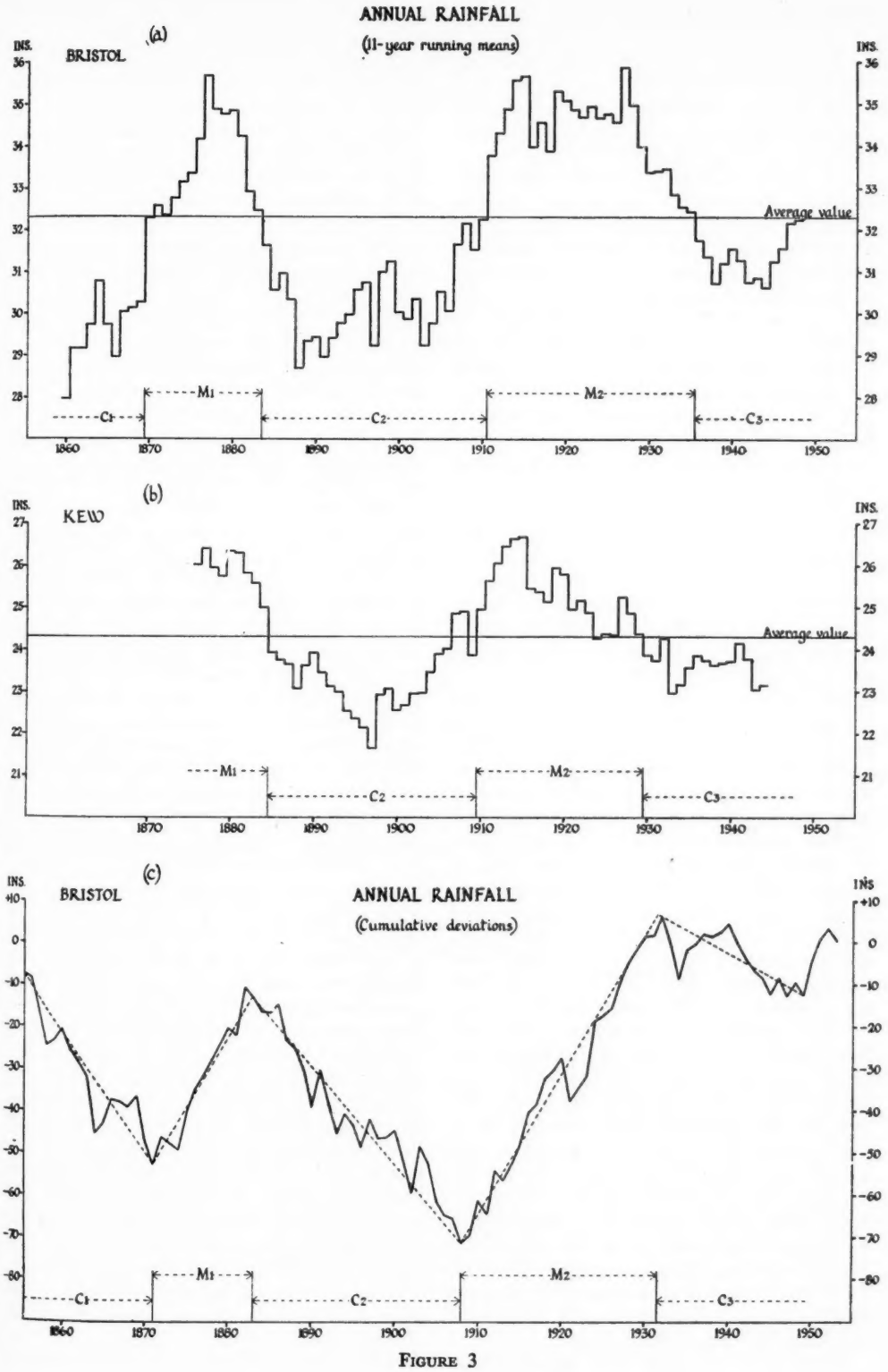

ANNUAL RAINFALL
(a)
BRISTOL
(11-year running means)
INS.
Average value
M1
M2
C1
C2
C3
(b)
KEW
(c)
BRISTOL
ANNUAL RAINFALL
(Cumulative deviations)

FIGURE 3

in the computation of that value that the 1924 rainfall total was first used.

(iii) The running mean process applied to a random series in general gives rise to a series which is no longer random in character.

For these reasons it would seem that climatic fluctuations are better investigated by means of ' residual mass curves,' a device which is commonly used in hydrology to register an excess or deficit of stream-flow over a prolonged period, and whose application to the study of climatic fluctuations was pioneered by Keele (3) and Barnes (4). Such a curve for annual rainfall at Bristol is shown in Fig. 3*c*. In its compilation, the rainfall in each year was first expressed as a deviation, plus or minus, from the long-term average over the whole period. The value to be plotted against any individual year was then obtained by summing algebraically all the separate deviations which had occurred up to that year inclusive. Thus for this particular purpose the result is best described as a ' cumulative deviation curve,' since this term is descriptive of the process used in its preparation.

During a continental phase, in which the annual rainfall totals are in general less than the long-term mean, the cumulative deviation curve will slope downwards. On the other hand, it will rise during a maritime phase when the annual rainfall totals are in general greater than the mean. It is at once seen that Fig. 3*c* is very readily divisible into such separate sections, the mean slopes of which are indicated by the broken lines. Moreover, the exceptional years in each phase now stand out very clearly. Thus 1903, notwithstanding that it is included in a continental phase, was a particularly wet year, whilst 1921 in the second maritime phase was notably dry. Such years are no longer lost in the general trend, as was the case on the corresponding graph of running means (Fig. 3*a*). Furthermore, the actual recording in any year can easily be read off the curve. Thus, since the 1903 and 1921 plots are respectively 10 inches above and 10 inches below the previous points, it is at once apparent that the annual rainfall totals in each of these years was 10 inches more and 10 inches less than the long-term mean.

There may be some who would argue that since such a curve is unsmoothed, one is perhaps more conscious of these minor irregularities than one should be when considering general trends. However, there will surely be others who favour a device which constantly reminds us that each and every phase does in fact embrace such exceptional years. In short, one is of the opinion that the great advantage of these cumulative deviation curves lies in the fact that they present a clear picture of general trends without in any way obscuring the exceptional recordings. Moreover, the dates of phase change can now be read off with much more certainty, and those represented in Fig. 3*c* differ from the corresponding points on the running means graph (Fig. 3*a*) by 2, 0, 3 and 4 years respectively.

Figs. 4*a* and 4*b* show the cumulative deviation curves for winter temperatures at Bristol and at Kew. When it is borne in mind that these curves, unlike graphs of running means, are unsmoothed, the degree of similarity between them is quite remarkable, for every single kink on the one is repeated in exactly the same position on the other. That minor interruption in the second maritime phase which occurred between 1905 and 1909 is well represented on both, as also is that short succession of maritime winters which interrupted C_3 between the years 1934 and 1939. In fact, the only difference is that the second continental phase seems to have lasted for two years longer at Bristol than at Kew. This of course is entirely dependent on one's interpretation of the mean slope of each curve during that phase, and the relevant broken lines could undoubtedly have been drawn with more accuracy if readings for earlier years had been available. The next phase change occurs in 1928 on both curves.

With regard to summer temperatures, it is again found that there is a marked similarity in general trend between the cumulative deviation curves for Bristol and Kew (Figs. 5*a* and 5*b*). The major points of phase change occur in exactly the same positions on both, and in fact the only significant difference concerns the section between 1902 and 1918. In the case of Kew this can be firmly included in the second maritime phase, whereas at Bristol it represents a period of normal

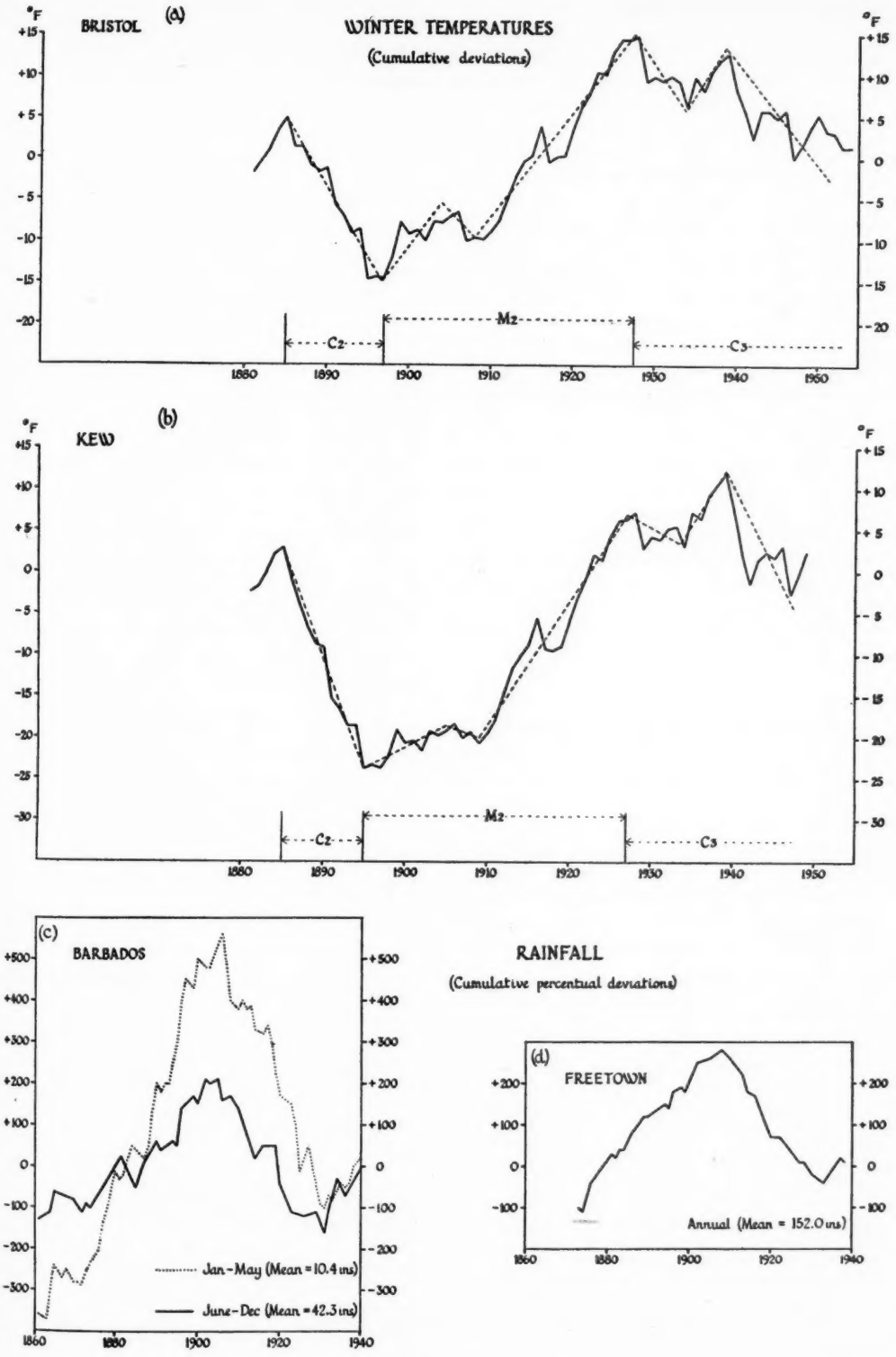

(a)
BRISTOL
WINTER TEMPERATURES
(Cumulative deviations)
°F
+15
+10
+5
0
-5
-10
-15
-20
C2
M2
C3
1880
1890
1900
1910
1920
1930
1940
1950
1960
(b)
KEW
(c)
BARBADOS
+500
+400
+300
+200
+100
0
-100
-200
-300
Jan.-May (Mean = 10.4 ins)
June-Dec (Mean = 42.3 ins)
1860
1880
1900
1920
1940
RAINFALL
(Cumulative percentual deviations)
(d)
FREETOWN
Annual (Mean = 152.0 ins)

FIGURE 4

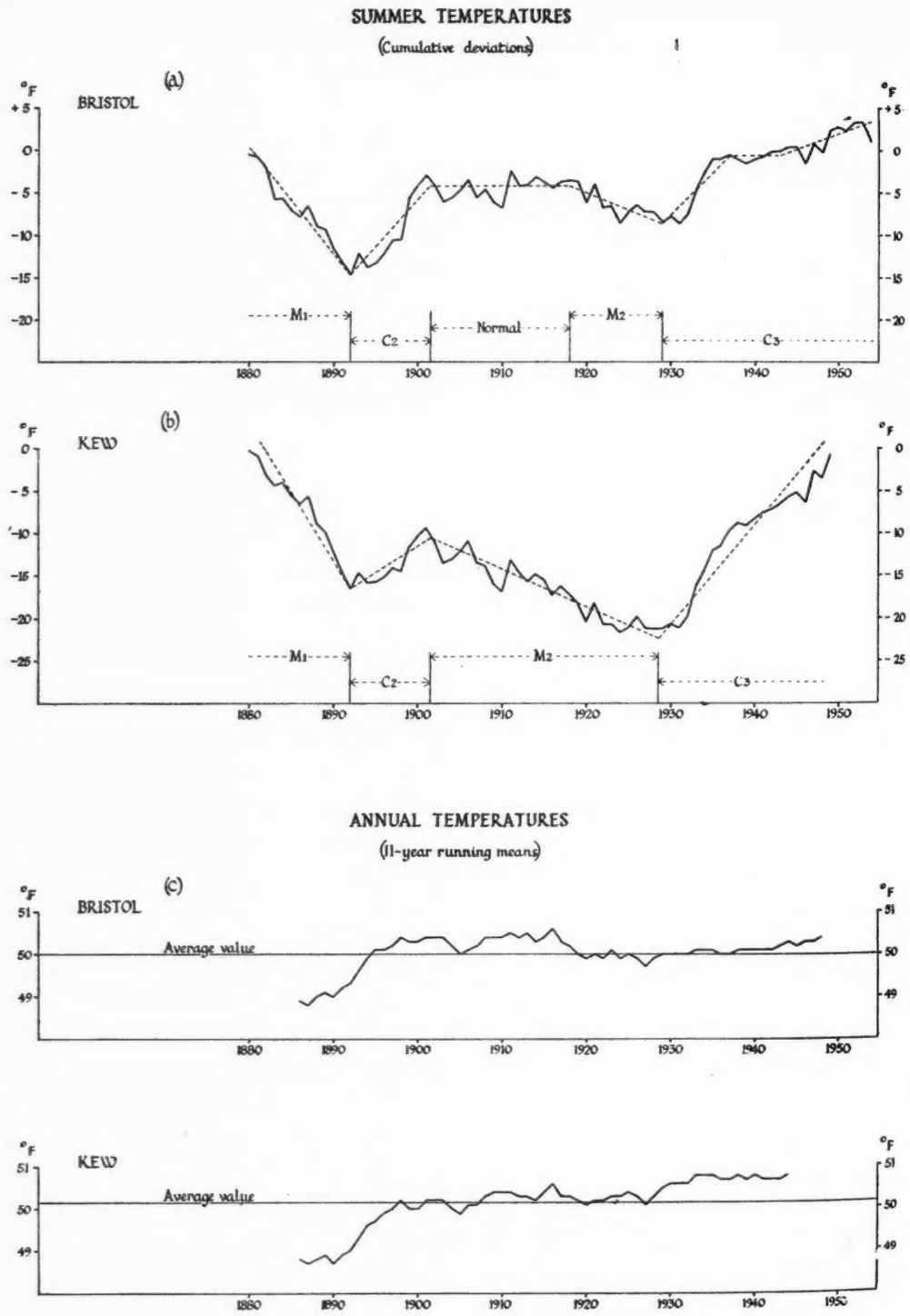

SUMMER TEMPERATURES
(Cumulative deviations)
(a)
°F
BRISTOL
+5
0
-5
-10
-15
-20
M1
C2
Normal
M2
C3
1880
1890
1900
1910
1920
1930
1940
1950
(b)
KEW
0
-5
-10
-15
-20
-25
M1
C2
M2
C3
ANNUAL TEMPERATURES
(11-year running means)
(c)
BRISTOL
51
50
49
Average value
KEW
Average value

FIGURE 5

summer temperatures before that phase begins. A shorter period of normal summer temperatures which was experienced at Bristol between 1937 and 1943 is also not represented on the Kew curve.

The summary of temperatures which best indicates phase changes is obtained from a consideration of summer minus winter values. These will obviously be above normal during a continental phase, when summers are hot and winters are cold, and below normal during a maritime phase, when summers are cool and winters mild. The cumulative deviation curves of summer minus winter temperatures at Bristol and at Kew (Figs. 6*b* and 6*c*) again display a remarkable degree of similarity both in general and in detail. Not only are the points of phase change identically placed, but every single kink on the one is repeated in exactly the same position on the other.

A detailed study of Figs. 6*a* and 6*b* shows that the following periods can be selected as representative of the various phases :

C_1—from 1855 (and perhaps earlier) to 1871, for which period rainfall figures only are available.

M_1—from 1872 to 1885, which year is marked by a very abrupt change of slope on the temperature curve. Temperature figures are available only for the last 6 years of this period.

C_2—from 1886 to 1901.

M_2—from 1912 to 1928.

C_3—from 1932 to 1949, after which year the rainfall curve begins to rise once more.

The figures given in the table below represent the average values of rainfall and temperature in each of the above periods expressed as deviations from the long-term means, and they emphasise the reality of those phase changes which are postulated.

During the continental phases the average rainfall for winter, summer and the year as a whole is in each case below the long-term mean, whereas in the maritime phases all average rainfall figures are above those mean values. During C_2 and C_3, the only continental phases for which temperature figures are available, winters were cold and summers warm. On the other hand, during the two maritime phases the average winter temperatures were well above the long-term mean, whereas those for summer were below. The deviation in mean annual temperature for each phase shows the same sign as the winter or summer figure depending on which of these is arithmetically the greater. Thus in M_1 and C_2 it was negative, whereas in M_2 and C_3 it was positive.

Before any attempt is made to offer a possible explanation of the cause of these climatic fluctuations there are five very significant facts to which attention must be drawn :

(i) Graphs showing 11-year running means of annual temperature (Figs. 5*c* and 5*d*) at once indicate that there has been a marked similarity in trend as between Bristol and Kew. Such minor differences as are noted can readily be

Phase	Period		Winter	Summer	Annual
C_1	1855–1871	Rain	−1·0″	−0·7″	−3·1″
		Temp.....	—	—	—
M_1	1872–1885	Rain	+0·5″	+0·7″	+2·6″
	(1880–1885)	Temp.....	+1·0°	−1·1°	−0·8°
C_2	1886–1901	Rain	−0·9″	−0·5″	−2·2″
		Temp.....	−0·9°	+0·3°	−0·3°
M_2	1912–1928	Rain	+2·1″	+0·9″	+3·5″
		Temp.....	+1·3°	−0·3°	+0·2°
C_3	1932–1949	Rain	−0·2″	−1·0″	−0·8″
		Temp.....	−0·3°	+0·6°	+0·3°

Average values in each period expressed as deviations from the long-term means.

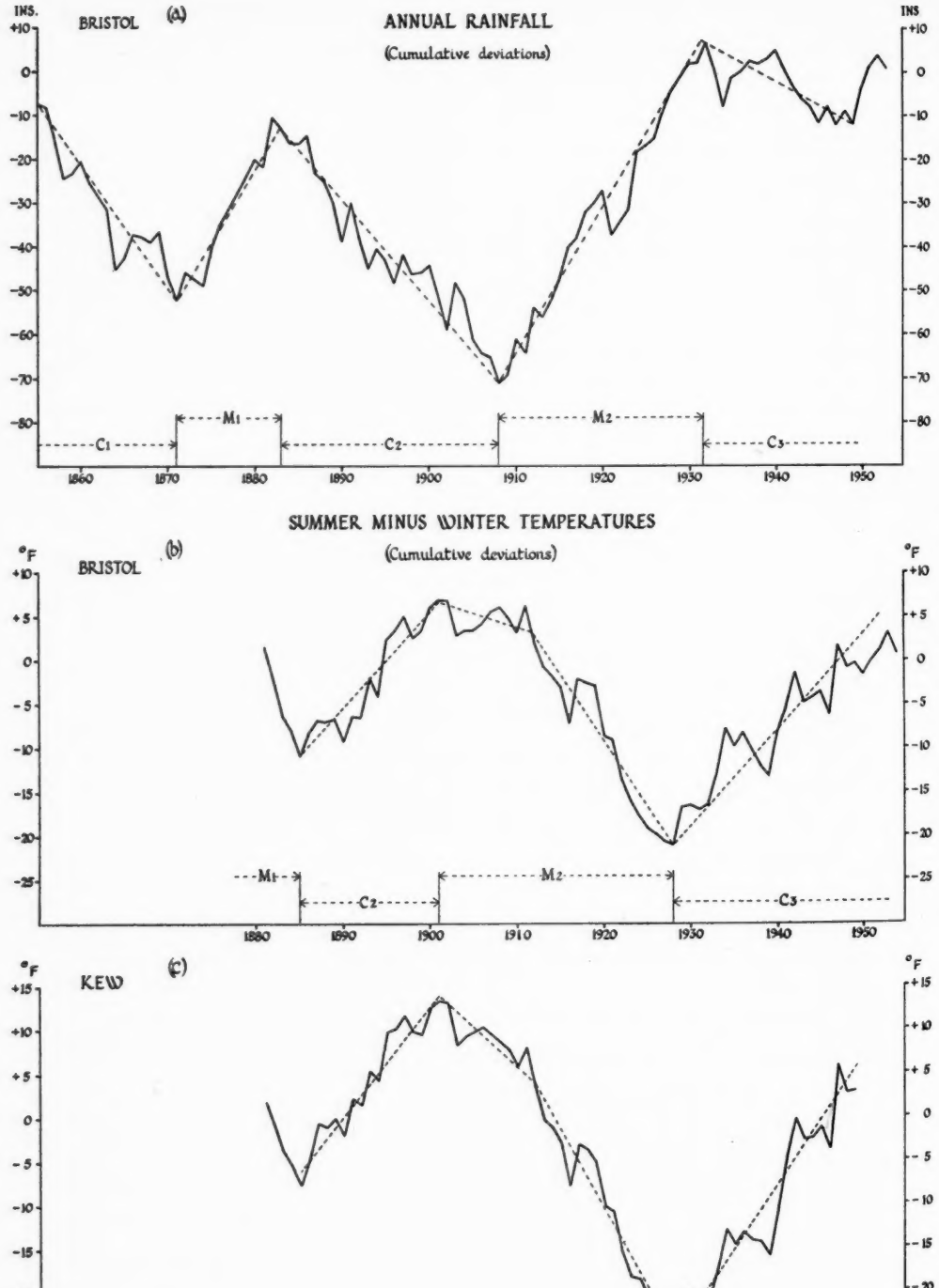

INS.
BRISTOL
(a)
ANNUAL RAINFALL
(Cumulative deviations)
INS
+10
0
-10
-20
-30
-40
-50
-60
-70
-80
C1
M1
C2
M2
C3
1860
1870
1880
1890
1900
1910
1920
1930
1940
1950
SUMMER MINUS WINTER TEMPERATURES
°F
BRISTOL
(b)
(Cumulative deviations)
+10
+5
0
-5
-10
-15
-20
-25
M1
C2
M2
C3
1880
1890
1900
1910
1920
1930
1940
1950
°F
KEW
(c)
+15
+10
+5
0
-5
-10
-15
-20
-25
-30
M1
C2
M2
C3
1880
1890
1900
1910
1920
1930
1940
1950

FIGURE 6

explained by comparing these graphs with others showing 11-year running means of summer temperature.

(ii) In his opening address to the panel on Climatic Change at the 1953 Toronto Meteorological Conference, Professor G. Manley referred to the surprising similarity in temperature fluctuations on both sides of the North Atlantic. This relationship has since been demonstrated by G. S. Callendar, who compared the 10-year running means of annual temperature at Kew with those for a group of five stations in Southern Ontario (5). When the great distance and also the very considerable differences of climate between these two regions are taken into account, it is very remarkable that the decadal temperature fluctuations should be so nearly concordant.

(iii) E. B. Kraus has prepared cumulative percentual deviation curves of annual rainfall for a number of tropical stations distributed over four continents (6), and those for Barbados and Freetown are reproduced in Figs. 4*c* and 4*d*. After a detailed analysis, Kraus concludes that such curves for all his tropical stations show a marked increase of annual rainfall from 1860 to the turn of the century, then an equally sharp decline to about 1930, and a subsequent recovery. At Barbados and Freetown these major reversals of trend occurred in 1906–8 and 1931–3. It is seen from Fig. 3*c* that the dates when such changes occurred at Bristol (1908 and 1932) agree exactly. However, if the phase M_1 be temporarily ignored, it is evident that Bristol experienced a sharp *decrease* in its annual rainfall totals from 1860 to 1908, a marked *increase* from 1908 to 1932, and a subsequent *decline*. In other words, the trends at Bristol have been precisely the reverse of those for Kraus's tropical stations.

(iv) C. E. P. Brooks and J. Glasspoole undertook a detailed study concerning certain exceptionally dry periods of several months duration which have from time to time affected the British Isles (7). Two of these occurred in what is here described as the first continental phase, four in the second continental phase, and a seventh in 1921, that exceptional year of continental character which entrenched itself firmly into the middle of the second maritime phase (Fig. 6*a*). During these particularly dry periods the rainfall of England and Wales as a whole was no more than 38–68 per cent. of the average, but it is very significant that in every case the drought in Scotland was less severe. What is much more vital in connection with the explanation of climatic fluctuations offered below is the fact that each of these periods of drought was characterised by a positive pressure anomaly over the British Isles. This was never less than 2·5 mb. and in some cases it exceeded 5 mb., thus giving the mean figure of 4 shown in Fig. 7*a* (after Brooks). It is particularly worthy of note that the belt of positive anomalies extends eastwards to North America, and that during these periods when pressure was above normal over the British Isles it was *below* normal over Barbados, Freetown and the tropics in general and also over the Arctic.

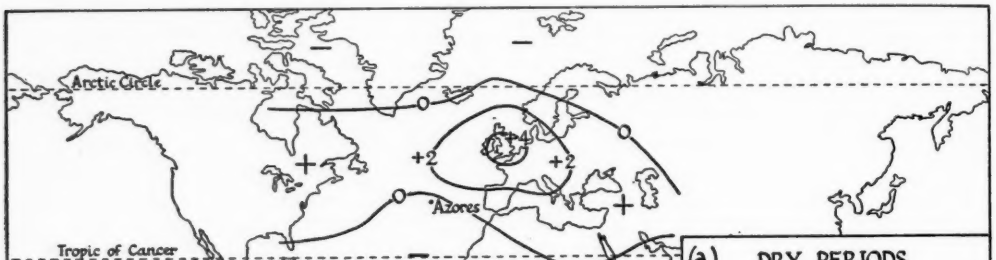
(v) Brooks and Glasspoole later studied seventeen exceptionally wet periods of several months duration which have from time to time affected the British Isles in general (8). Six of these occurred in what is here described as the first maritime phase and seven in the second maritime phase. Of the remainder, one occurred in 1891 and two in 1903, those exceptional years of maritime character which intervened during the second continental phase (Fig. 6*a*). During these particularly wet periods, of which the winter of 1911–12 was a good example, the rainfall of England and Wales as a whole was about 150 per cent. of the average. In the southern half the figure sometimes exceeded 200 per cent., but the rainfall of North-west Scotland was on most occasions less than normal. What is much more significant in connection with the explanation which is to follow is the fact that each of these periods of heavy rain was characterised by a negative pressure anomaly over the British Isles of at least 2·5 mb. The centre of the area of negative pressure deviations was frequently located to the west of Ireland, where the anomaly was sometimes as great as —10 mb. (Fig. 7*b*—after Brooks). It is particularly noteworthy that the zone of negative pressure anomalies again extends westwards to reach North America, and that during these periods when pressure was below normal over the British Isles it was

above normal over Barbados, Freetown and the tropics in general and also over the Arctic.

All those climatic fluctuations which have been experienced at Bristol could be accounted for by alternate northward and southward migrations of the Azores high-pressure belts shown in Fig. 1. When these are displaced to the north, stations in Southern England and Wales at least will experience continental phases both

Bristol and Southern Ontario agree so very closely, for a northward or southward displacement of the high-pressure belts would obviously have the same general effect at all three locations.

If the Azores high-pressure belts were generally displaced to the north of their usual positions from 1855 to 1908, this would explain why the annual rainfall totals for such stations as Barbados and Freetown were above normal during that



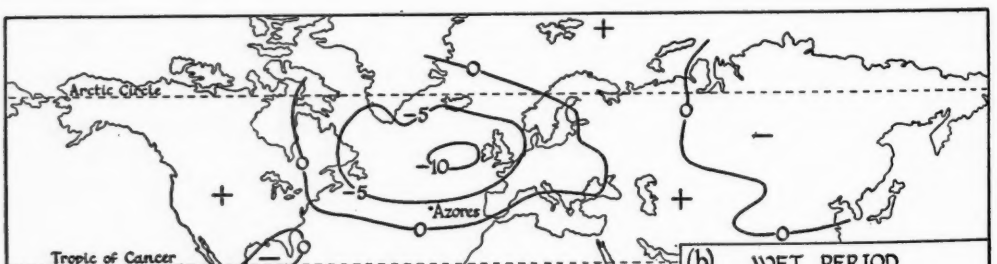


FIGURE 7

in winter and summer, whereas, when they lie to the south of their normal positions, maritime conditions will prevail. The position of the 30-inch isobar in both seasons makes it readily apparent that only minor shifts would be necessary to bring about such changes, at least as far as Bristol and Kew are concerned.

Moreover, such migrations of these northern high-pressure belts would provide an explanation for each of those five significant facts specified above. No longer is it difficult to understand why the mean annual temperature trends at Kew,

period, for the inter-tropical zone of convergence would have been free to move further north. Meanwhile, the annual rainfall totals for Bristol should have been below normal, and this was certainly the case in all but 12 years of that 53-year period. It is particularly worthy of note that the increase in annual rainfall totals at Bristol from 1871 to 1883 (Fig. 6*a*) was due primarily to excessive falls in autumn, as is evidenced by the fact that the positive deviation for that season was as high as 1·8 inches, whereas the corresponding figures for winter and summer were only

0·5 inch and 0·7 inch respectively. In other words, the short intervening phase M_1 was due much more to excessive cyclonic activity in the transitional season of autumn than to a marked predominance of maritime influences in winter and summer.

If the seasonal high-pressure systems of the northern hemisphere were displaced to the south from 1908 to 1932, this would explain why the cumulative deviation curves of annual rainfall for Barbados and Freetown declined so steeply between those years whilst that for Bristol showed a concurrent and equally marked rise. After 1932 these high-pressure systems may have moved north once more, and this would explain why the annual rainfall at Barbados and Freetown increased whilst that at Bristol declined.

If the northern high-pressure systems move as postulated above, it would seem reasonable to suppose that those of the southern hemisphere migrate also. In his paper on tropical rainfall regimes (6), Kraus shows that during the first three decades of the twentieth century tropical stations distributed over no fewer than four continents suffered not only a decline in their annual rainfall totals but also a marked shortening of the rainy season. This would seem to imply that whilst the northern high-pressure systems were displaced to the south, those of the southern hemisphere had moved north. Such migrations would have resulted in a narrowing of the inter-tropical zone of convergence and thus a shortening of the rainy season at tropical stations. Turning to more recent times, there is evidence which would seem to suggest that the northern and southern high-pressure systems were also displaced towards the Equator during the summer of 1954 (9). Throughout Europe as a whole, that season was in general both cooler and wetter than normal, but its most persistent feature was the establishment of a large area with pressure below average which included most of Western Europe and the eastern Atlantic Ocean,[1] and which induced depressions to move along tracks which lay much further to the south than is usual. This would seem to indicate that the Azores high-pressure system was displaced to the south of its normal position. Meanwhile, the same months in South Africa, which there constitute the winter season, were also wetter and colder than normal and pressures again showed pronounced negative anomalies. This seems to suggest that the southern high-pressure system was also displaced towards the Equator. Corrobatory evidence is supplied by the fact that the monsoon rainfall of India in June and July was heavier than normal, as also was the rainfall of Malaya. This would be the expected result if the pressure gradients over those areas were increased consequent upon a northward displacement of the southern high-pressure system.

Such migratory movements of the Azores high-pressure system as are postulated above readily explain why all exceptionally dry periods in England and Wales are characterised by positive pressure anomalies. Moreover, it is no longer difficult to understand why the droughts in Northern Scotland are in all cases less severe, for that region could expect to be visited by at least some of those depressions which would be deflected northwards by the higher positive pressure anomalies over Southern England. Such a thesis also explains why periods of exceptionally heavy rain over England and Wales are all characterised by negative pressure anomalies. Furthermore, it is now readily understood why the rainfall of North-west Scotland is so frequently below normal when that for England and Wales is much increased. Depressions moving well to the south of their usual tracks would bring less rain to North-west Scotland and correspondingly more to England and Wales. There would seem to be no other theory which so simply accounts for the fact that annual rainfall totals in Northern Scotland and Southern England very frequently show deviations which are opposite in sign.

It may well be argued that any attempt to account for climatic fluctuations by some such theory of pressure migration begs the question, since one is now faced with the problem of what causes such migrations. To this there is as yet no answer, though it might perhaps be suggested that an increase in the velocity of

[1] In August mean pressure was below normal over a very extensive area including the whole of Europe and most of the North Atlantic.

the whirl of air around each Pole may result in the fact that pressure will go down in the centre and up at the sides. Thus when this vortex in the atmosphere around the North Pole becomes intensified the pressure in Arctic regions may be decreased whilst that in the surrounding temperate belt is increased. This would cause the Azores high-pressure system to move further north, thus altering fundamentally the climatic conditions of Southern England. Conversely, a decrease in the velocity of the wind vortex around the Pole might lead to a southward displacement of the Azores high-pressure system.

Mention has already been made of the fact that during our notably dry and wet periods pressure anomalies over the British Isles are opposite in sign to those of Arctic regions. In this connection it is particularly worthy of note that F. M. Exner found very significant negative correlations between winter pressure readings in the Arctic and the corresponding recordings at several stations both in Western Europe and in North America (7) (10). This led him to conclude that, at least in winter, high pressure in the northern temperate zone is very closely associated with low pressure over Arctic regions, and vice versa. Brooks, who compared summer pressure readings at Spitsbergen with those at Valencia on the south-west coast of Ireland, also found that the records gave a highly significant correlation coefficient of $-0{\cdot}78$ (7). It is a remarkable fact that G. C. Simpson, who was the meteorologist on Scott's 1910 expedition, obtained a similar result in the southern hemisphere (7). When he compared his monthly pressure figures at the Antarctic station with those in more temperate latitudes he found a marked belt of negative correlation lying to the north of latitude 60° S., in which the coefficients were as large as $-0{\cdot}5$, particularly for several places in South America. In other words, pressure changes as between South America and the Antarctic are as contradictory as those between northern temperate latitudes and the Arctic.

Whether or not there is any truth in this suggestion that changes in the velocity of the wind vortex around each Pole lead to movements of the high-pressure systems, one is reasonably confident that climatic fluctuations are very closely linked with changes in the upper atmosphere where these circumpolar whirls are located. Consequently, any further speculation must be dependent, firstly, on the future work of those meteorological specialists who study large-scale circulation patterns, and perhaps in particular on the efforts of those who deal with long-wave patterns in the zonal westerlies, and secondly, on fluctuation studies for many other stations situated in different climatic regions and scattered broadly over the earth's surface.

Acknowledgments

The author wishes to thank the Clifton College Librarian, the meteorological observer at the University of Bristol's Agricultural and Horticultural Research Station (Mr. G. E. Clothier), and the Bristol Waterworks Company for so readily placing at his disposal all the relevant recordings. He also acknowledges with gratitude the receipt of a financial grant from the Colston Research Society which met the cost of the illustrations.

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CEREBRAL ACTIVITY

On Friday, September 2, 1955, at the Bristol Meeting of the British Association, Section I (Physiology) and Section J (Psychology) jointly discussed the problems of 'Cerebral Activity.' Dr. C. G. Phillips of Trinity College, Oxford, opened with a contribution on nerve cells. He was followed by Dr. W. Grey Walter of the Burden Neurological Institute, Bristol, who presented a paper on electrical signs of mental function. Dr. D. M. MacKay of King's College, University of London, contributed a paper entitled 'Towards an information-flow model of cerebral organisation.' The session was concluded by Mr. Malcolm Piercy of the University of Bristol with a discussion of behavioural studies of cerebral dysfunction.

An edited version of each of these papers is printed below.

Nerve Cells

by

Dr. C. G. Phillips

Insight into the workings of the nervous system began with the discovery, towards the end of the last century, that it is built up of independent units, the nerve cells or neurones. Probably each one of us possesses about a hundred thousand million of these. In the cell body, which may measure up to 80 microns in its largest dimension, and in the nucleus, etc., nerve cells resemble other cells. But their important biological properties depend on their protoplasmic tentacles. Numerous branching, finely tapering dendrites spring from the cell body, extending sometimes to a length of a few millimetres. They may be radially symmetrical about the cell body, or they may grow mainly from one pole. Another process, the axone, breaks up into branches with specialised endings. Nerve cells are of two main types. In one of these, the axone is not much longer than the dendrites, and its endings are applied to the dendrites and bodies of neighbouring cells. In the other, the axone is longer : at most, as long as one's spine, or from loin to foot. Its endings are applied to the dendrites and bodies of distant central nerve cells, to nerve cells in outlying ganglia, or to the cells of our muscles.

The Spanish neurologist Ramon y Cajal, contemplating the structure of nerve cells, proposed with wonderful intuition that their function is to transmit signals inwards along the dendrites towards the cell body and thence outwards along the axone. Half a century's subsequent research has supported him. Signals from the sense organs, generated at the periphery, pass along nerve fibres which can be regarded as dendritic processes of cell bodies lying just outside the central nervous system. The axones of these cells (here continuous with the elongated 'dendrite ') transmit them centrewards. Here new signals are generated in many dendrites, and flow towards and along many axones. Eventually there may be a discharge of signals along axones passing out of the central organ to the periphery, resulting in reflex movements or secretions. The common effector apparatus also allows emotional expression, skilled movements, and the extreme refinements of speech and gesture.

How are nerve cells stirred into action, and what is the nature of the signals they transmit ?

Energy derived from the cell from its burning of foodstuffs is harnessed to maintain a voltage gradient across the plasma membrane investing its dendrites, body and axone. When no signals are being transmitted the cell interior is between sixty and ninety millivolts negative to the exterior.

This voltage has been measured between a micro-electrode piercing the cell membrane

and an external reference electrode. There is evidence that the cell pumps sodium ions outwards across the membrane, against the concentration gradient. Large anions are unable to pass outwards. As a result, potassium and chloride ions distribute themselves so that with more potassium inside the cell than outside, and more chloride outside than inside, the system comes into equilibrium with the cell interior negative to its exterior.

Signals arriving at the specialised terminals applied to the dendrites and body of a nerve cell probably cause the secretion of minute doses of chemical transmitter substances. The terminals are of two types. The secretions of *inhibitory terminals* tend to increase the voltage already existing across the cell membrane. The secretions of *excitatory terminals* diminish the membrane voltage. If this diminution is small enough, the change remains quite localised. But if the voltage falls below a critical level, a self-propagating signal travels away from the excited loci to the very ends of the cell : as far, for example, as from brain to lower spine, or from lower spine to foot. This transmitted signal is the *nervous impulse*. The speed of its transmission by our largest axones is about 120 metres per second.

When the excitatory process reduces the local membrane voltage below the critical level, current flows inwards through the excited loci and outwards through the adjacent membrane to complete local electric circuits. A sudden inrush of sodium ions causes the cell interior to become locally and momentarily positive. Then a rapid escape of potassium ions restores the previous internal negativity. This is the disturbance that travels along the whole length of the membrane, and leaves it ready to transmit another such disturbance. All but the smallest of our axones are cuffed with fatty insulating material, in which there are gaps at intervals of 1 to 3 millimetres. So the flow of current across the axonal membrane is confined to the gaps—the so-called nodes of Ranvier. Local circuits ahead of the active region of the advancing impulse are completed by currents flowing outwards through the nodes. Each node in turn responds by an inward surge of action current. Finally, as the impulse passes beyond it, each node is repolarised in readiness to transmit another impulse. The impulse, in fact, jumps from node to node along the whole length of the axone.

The currents flowing in the body fluids outside the cell and its processes during activity can be detected by suitable electrodes attached to amplifying and recording instruments. There is evidence that these currents may influence the excitability of neighbouring cells.

Thus the nerve cell has properties of two quite different orders. First come the local changes produced in dendrites and body by the secretions of the intercellular contacts with which their membranes are encrusted. The local *excitatory* process tends to fire impulses whose frequency (say ten to one hundred impulses per second, with a theoretical maximum of nearly one thousand per second) parallels its intensity. Opposed to this is the local *inhibitory* process which tends to prevent the firing of impulses. Second come the propagated signals, the nerve impulses, which speed away from their site of origin over the whole extent of the cell membrane.

These properties are the basis of the cell's participation as a unit in an information system. The cell functions as a relay vastly more complex than the simplest physical relays. For integration of excitatory and inhibitory processes at hundreds of dendritic contacts allows a wide and delicately graded range of response. The value of the membrane voltage at any instant determines the amount of further excitation needed if the cell is to be made to fire. It may be poised near to the critical firing level, or it may be ' biassed off,' over a range of about twenty millivolts, by preponderating inhibitory action. And at higher intensities of excitation, the frequency of impulse firing is variable over a wide range. If the cell is firing impulses for much of the time, as many neurones in the brain seem to be doing, the rate of firing can be accelerated by excitatory action, and slowed or halted by inhibitory action.

The time scale of all these events is a scale of milliseconds. But this is not the time scale by which behaviour is measured. There is evidence that repetitive activity causes an enduring facilitation of transmission from cell to cell. Possibly this is due to structural changes in the intercellular contacts. It is not hard to imagine that such changes could impose more or less definite pathways on a previously diffuse system of interconnections.

Our understanding of the principles of nervous integration came at first from Sherrington's analysis of reflex action, which was guided all along by contemplation of nerve cell structure. Direct experiments on nerve cells came later. We can trace certain reflex pathways through brain-stem and spinal cord, and follow impulse traffic in certain other well marked central nervous tracts. But beyond this we cannot at present go. We could not, for example, trace in detail the structural basis and impulse traffic of speech or of skilled movement. It is likely, indeed, that specific configurations of activity, occurring in one and the same general brain region, may engage different populations of brain cells on different occasions, instead of being tied to an unvarying anatomical substratum. In default of the possibility of exact knowledge we need not remain idle. We can construct theoretical models of central nervous organisation, based on known properties of the nerve cell units. The time will come when, with increased understanding and technical refinement, we shall be able to see whether any of these models have recognisable counterparts in real brains.

Electrical Signs of Mental Function

by

Dr. W. Grey Walter

Empirical observations in laboratory and clinic before the war, enabled us to make electroencephalography a conventional aid to diagnosis of diseases of the brain; we are now able with more elaborate equipment to trace in some detail the electrical changes in the brain that accompany perception, thinking and action, and to chart the electrical patterns that characterise individual mentalities.

In normal people, it is convenient to distinguish between two classes of electrical activity: 'spontaneous' rhythms, and responses evoked by stimulation. The origin and precise significance of the spontaneous rhythms are still uncertain, but a fertile hypothesis is that they serve to select, arrange and distribute the enormous quantity of information which reaches the brain from the sense organs. According to this hypothesis the types of spontaneous activity called 'alpha rhythms' which have long been considered as associated in some way with vision and visual imagination, are analogous to the scanning waveforms of a television system. This comparison helps to explain why the brain activity at first seemed so 'constant'; the early records were taken as simple graphs of voltage changes with respect to time, so that only the larger, more regular components were detectable. A television signal also, displayed as a simple voltage-time graph, appears to consist mainly of regular timing pulses, the picture-signal itself being blurred and irregular. It is only when the whole signal is separated into 'administrative' and 'operational' components that the detail and significance of the original pattern is re-established.

In the case of the brain, classification of the electrical signals in this way is still difficult. No two brains are quite the same. The human brain never repeats itself and the whole system might have been specially designed to evade scrutiny. Nevertheless, recently developed methods of frequency- and phase-analysis (demonstrated to parties visiting the Burden Neurological Institute) have shown that the general pattern of spontaneous alpha rhythms is related to personal characters of mental imagery and versatility and that in any particular person the momentary details of alpha activity in the various regions of the brain are connected with the nature and significance of incoming signals and with the level of internal attention and association.

The correlation between brain rhythms and behaviour is made more difficult by the inverse relationship between electrical amplitude and regularity on the one hand and the intensity and diversity of behaviour on the other; the more vivid and varied a person's experience and imagination, the more inconspicuous and variable are the so-called spontaneous or administrative rhythms. There is the further complication that the alpha activity is always plural; there are many rhythms in this category and each may have its proper function. These effects, however, are included in the predictions of the scanning hypothesis and encourage its

extension to the problem of how external sensory patterns are compared in the brain with previously stored images to permit selection, recognition and learning. The great differences between people in all these respects, the similarities in uniovular twins and the constancy of the pattern in normal persons suggest that these characters are inborn rather than acquired and it is possible to group normal subjects according to the patterns of responsiveness of their spontaneous rhythms and their habitual modes of imagination.

On the basis of this tentative scheme of classification there have been observed signs of affinities and repulsions between people of similar and discrepant brain types respectively. There is some evidence that these aspects of brain function are connected with those features of human character that influence the formation of personal and social relations. Couples with similar types of brain activity (for example, lack of alpha rhythms) who by definition should also employ and enjoy vivid visual imagery, tend to associate readily with one another and soon find themselves on intimate terms. Possessing similar views on the tactics of everyday life, they can live and work together without having to make great concessions to one another's habits of thought, even if there is a divergence of strategic aim. On the other hand such 'supplementary pairs' tend to make the same sort of mistakes and cannot criticise one another's point of view. Complementary pairs, who have discrepant brain activity and imagery, have more difficulty in coming to close personal terms and may feel ill at ease with one another but when they do succeed for strategic purposes in setting up a line of communication—by writing or drawing or through a go-between of intermediate type—they form a system with mutual error correction and may create a superior type of joint enterprise. In cases where there is both tactical and strategic incompatibility there is usually a marked and permanent antipathy and revulsion, but the electrical behaviour of the brain has not so far revealed evidence relating to aim, but only to methods of aiming.

This approach to personal relations, though obviously and deliberately ingenuous, has the virtue of explicit prediction. For example, it would be expected that, in the case of a couple with inborn tactical affinity, who are accustomed to agree readily in their daily work, the appearance of strategic discord would produce deep emotional disturbance and mutual distrust. Contrariwise, a complementary pair who are accustomed to see common problems from different points of view would not be so disillusioned by divergence of interests and allegiance. These considerations apply both to working teams and marital association, and experiments are now in progress to test the value of such predictions. The experimental methods involve the recording of brain potentials simultaneously with several other physiological variables both during tranquillity and during the solution of a series of varied problems, some of which involve social judgment, as during the playing of a game. At the same time the subjects' method of learning is studied by an application of Pavlovian conditioning techniques. These observations are then compared with the results of psychometric tests and social conduct. The statistical analysis of the objective records and psycho-social behaviour presents considerable difficulty, but there is no reason to suppose that the problem is quite intractable; some analogue-type statistical computors have been made to shorten the computations.

Apart from the study of changes in the spontaneous, administrative rhythms, experiments with special forms of flickering light demonstrate how the pattern of a stimulus situation is analysed by the brain in terms not only of Gestalt but of statistical significance. The technical difficulty of sorting out the evoked from spontaneous and random activity has been reduced by an elaborate system of topographic display, the Toposcope, in which a cluster of twenty-two brilliance-modulated cathode-ray tubes with a common revolving time base presents the electrical changes in the brain as recognisable patterns on a chart of the head. Automatic photography is used to integrate invariant components over a period of a second or so and in this way regular features in the responses to visual stimuli are often seen in regions far removed from visual projection areas, particularly in the temporal

lobes. These remote responses are usually distorted or embellished as compared with the primary visual ones; their distinctive characters appear only if the stimulus pattern is novel or significant in the statistical sense. Furthermore, the responses evoked in non-visual regions by visual pattern stimuli often persist for some seconds at least after the stimulus is over, and the length of this 'memory trace' also depends on the statistical characters of the stimulus. All these effects are blended with the spontaneous rhythms and they mirror in fine detail some of the differences and similarities between individual subjects.

Most of the diffusely projected responses to stimulation are probably due to signals transmitted through the diffuse reticular system in thalamus and brainstem. This diffuse, non-specific, afferent system has been studied mainly in anæsthetised or mutilated animals and it is difficult to compare such preparations with normal human subjects, but the evidence suggests that all signals reaching the brain from the sense organs are relayed to many cortical regions 'to whom it may concern, for information only,' and experiments such as those described above suggest that this process is an essential preliminary operation in the mechanism of learning by association, whereby the brain distinguishes between trivial or random events on the one hand and startling or significant ones on the other. A useful proposition derived from these observations is that 'pattern-recognition' and 'learning' are practically synonymous and both depend upon a statistical appraisal of experience.

Modern hypotheses about brain function are more sophisticated than in the days when the nervous system was compared with an immense telephone exchange; they follow the fashion of Communication Theory and include such concepts as self-adjusting filters which set themselves according to their experience of the signals which they receive. It would seem that, in these terms, the nervous system may be thought of as a stochastic rather than a logical machine. Study of the psychophysiology of maturation should provide information to test this notion, since the newborn brain would be expected to make the minimum number of assumptions about itself and its environment and to build up a set of working hypotheses on the basis of increasingly complex assessments of the contingency of experienced events. If we need an up-to-date mechanical model to help us to think about thinking it would have to be more like a totalisator than a telephone exchange.

From the complexity of the subject it is evident that no tenable hypothesis regarding the signals which we can observe passing to and fro in the brain can be based on any means of communication familiar to us in everyday life. Even if we assume that great aggregations of the cellular units of the brain operate together and on that supposition reduce the number of elements of communication from ten thousand million to a few thousands, the problem of communication by contact, either by nerve or by electrical pathways, is still intractable by present methods. We do not know whether any such degree of simplification is warranted. We do know that all the vital requirements of individuality and plasticity could be satisfied by a surprisingly small number of units in a plenary system: one thousand would be an over-generous allowance for a first-class brain permitting far more permutative combinations than there are particles in the Galaxy. The elaboration of cerebral functions may well derive not so much from the enormous number of elements in the brain as from the potential richness of their intercommunication.

Even so, after reducing the number of elements, any theory that intercommunication is by simple physical contact, by nerve fibrils or by established electrical pathways from cell to cell, still presents serious difficulties. Nor can the problem be simplified by assuming greater limitation in the richness of intercommunication; on the contrary the increasingly clear revelations of such activity which the toposcope is yielding every day indicate that there is an internal or domestic information service even more flexible than was previously suspected; all parts of the brain appear to be served alike with all information received by any one part. Repeated laboratory experiments show signals passing incessantly from one part

to another and the different manner in which each part deals with it. Before the days of wireless communication, anyone observing these activities of the brain and aware of the problems of contact communication, must have been driven to explain it by some transcendental conception, or some convenient generalisation such as the hypothetical independent activity of 'mind.' Knowing what we do today about the possibilities of distant communication and computation, and the reciprocal translation of the signal-in-time to the signal-in-space of television, and putting alongside this what we now know about visual scanning and what appears to be a related scanning by the electric rhythms of the brain, we seem to be on the verge of a rational theory to relate the physical events in the brain to the dimensions of mentality.

Towards an Information-flow Model of Cerebral Organisation

by

Dr. D. M. MacKay

1. Introduction

1.1. By far the greater proportion of fruitful work on the brain, as illustrated by the other three papers in this symposium, has been of an empirical rather than a theoretical nature. This is scarcely surprising; for the form of a theory must wait upon the terms in which we agree to describe the activity that it is intended to explain. And the trouble with the brain is that we lack even yet a generally agreed conception of what it does. We all *know*, of course, what it does. The problem is to put this knowledge into illuminating words which adequately present the task that we want to set the theorist.

1.2. Much theory-building in the past has framed its descriptions of brain function in terms of particular analogies which proved to be too concrete and specific. In the era of clockwork, the theorist considered his problem to be essentially that of devising a clockwork system to function like the brain. With the coming of the telephone, the problem became one of designing a telephone exchange that would perform in the same way. More recently we have had the digital computing machine in the same role. In each case the eventual conclusion was that these ways of describing brain function, although each partially justified, were too specialised and restrictive. The descriptive terms chosen were not sufficiently fundamental.

1.3. The present paper deals with some aspects of yet another theoretical approach, which is doubtless unlikely to avoid all the pitfalls of its predecessors. It differs from them, however, in seeking to frame its descriptions of brain function in more fundamental and general terms, which may hope to outlast changes in engineering fashion.

Recent years have seen the growth of a general theory of 'self-guiding information-flow systems' (1), in terms of which the most characteristic features of cerebral activity find ready description.

The theory enables us first to describe in general terms the characteristic regularities and stable features of the information pattern received by an organism from the world to which it must react adaptively; next, without committing ourselves to any particular mechanical model, we can work out the theoretical principles which any adaptive system must somehow embody if it is to take advantage of these regularities in the same way as the human organism appears to do.

Then, having defined in this way the class of possible models, we can proceed in the usual hypothetico-deductive way to test and refine particular models embodying these basic principles, doing our best to ensure that our theoretical model-making shall be strictly disciplined by the physiological facts.

1.4. The general principles of this approach I have discussed before an earlier meeting of this Association (2). In the present paper I should like to illustrate the way in which it works out, by considering in these terms some of the problems connected with the *development of organisation* in the central nervous system.

2. Self-organisation by Trial and Error

2.1. It is fairly easy to imagine a mechanism which can adjust what it is doing, according to its own success or failure, in such a way as to behave 'adaptively.'

The stock example is the thermostat, in which a mechanical thermometer indicates success or failure (in holding the temperature steady), and the movement of the indicator is used to turn the heat on or off accordingly.

In the normal thermostat the information from the indicator is sufficiently detailed to prescribe the correct adjustment uniquely. It indicates not only 'failure,' but also whether 'failure' means 'too high' or 'too low.' But suppose we had to use an indicator which left us guessing whether 'failure' meant that 'more' or 'less' heat was needed. We would not be stumped; but we would have to be prepared to allow more than one trial-attempt to correct the 'failure.' If turning on *more* heat did not correct it, the mechanism would next have to try turning on *less*. It could make up for the lack of information from the indicator, by increasing the average *number* of trials per successful correction.

2.2. This is all very well when the system has only two possible courses of action between which to select. If, however, it has more than two, the average number of trials per selection goes up accordingly. If, for example, we had to hunt at random for the right combination of settings of ten controls, each with ten possible positions, we should need an average of some 5×10^9 trials, if after each trial the indicator showed only 'success' or 'failure.' If in addition the environmental conditions are changing so that the action required changes as the trial process goes on, the hopelessness of the situation is apparent.

So although, in principle, it is possible for a self-organising system to grope its way to a most complex state of organisation given only the crudest indication of success or failure, in practice the time taken to do so will rise astronomically as the number of 'degrees of freedom' (the number of independent adjustments to be made) increases.

2.3. Now although the details of any self-organising flow-system to behave like the brain are vastly more complex than this example suggests, the basic principle still applies (3, 4). The number of possible trial-combinations at any stage in development must be commensurate with the amount of information which each trial provides. If there are too many alternatives to be explored, or too little information, the system cannot hope to deal successfully with a changing environment.

Since the human organism as a whole appears to possess several hundred nominally independent degrees of freedom, each with perhaps a dozen or two distinguishable levels of activity, it might appear quite impossible that its final organisation, representing one out of perhaps 10^{500} alternatives, could result from trial and error during development, even allowing for the fact that the broad lines of such organisation are genetically determined.

But we have been neglecting two considerations which change the picture completely.

3. The Regularities of the Environment

3.1. The first factor to take into consideration is the enormous degree of coherence and regularity in the world of an organism. In the jargon of Information Theory, the information from the environment has a high 'redundancy'; given one sample, you could guess nearly the whole of the next. (You are not surprised for example, to see the same room surrounding you each time you raise your eyes from this page.)

To take advantage of this statistical regularity, the theory suggests that trial processes should be internally *organised* to match the structure which it reveals.[1] For example, if two states of adjustment, 'A' and 'B', are found to be required alternately, the system could embody an 'organiser' whose job is to select 'B' after 'A' and vice versa.

This organiser in a sense *imitates* the regular feature of the environment to which it organises the response. It forms in effect an internal *representative* of this abstract feature of the world.

A system of organisers interlocked in the same pattern as the corresponding features of the world of activity would thus form effectively an internal *representation* of that world. *Perception* of a

[1] A more detailed discussion of the relation between redundancy and the information content required per trial is given in reference (3).

particular feature, on this model, would be represented by the arousal of the corresponding organising system.

3.2. Now there is no need to imagine such a system of organisers to be 'prefabricated.' Without going into details, it is evident that the principles of trial-and-error already discussed could enable the appropriate organising system to be discovered from experience (2, 3, 4), given sufficient time. The question, as before, is how long a time would be required. Could development on these lines ever take place as rapidly as it does in a human infant?

Again, we must not forget the broad guidance which we know to be supplied genetically. But even allowing for this, some further principle is required to make feasible the detailed elaboration of human faculties as a result of experience.

4. Development by Selective Differentiation

4.1. Once again, the principle that seems most promising is suggested by information theory. It applies to trial-and-error strategy in all fields, including that of theory-making itself! The basic point is that the selection of one out of a number of possibilities requires fewer trials, the more nearly *equal* are the probabilities of the possible outcomes at each trial.

So in our case, where the system must discover one out of a large number of possible arrangements, each of which has a far higher initial probability of 'failure' than of 'success,' it will pay to begin the process by grouping large numbers of possibilities together for purposes of trial, in order to raise the probability of 'success' at each step to something more comparable with that of 'failure.'

4.2. In practical terms, this means that instead of beginning development with a large and fixed number of degrees of freedom, the system should begin with large groups of its controls coupled together by means of adjustable 'statistical links,' which initially have a high probability of functioning (so that, for example, independent control of each of five fingers is not possible). The varieties of activity possible will then initially be clumsy and relatively inefficaceous, but they will be fewer in number, and the outlines of a suitable organising activity can the more quickly be developed to match the more gross regularities discovered in experience.

4.3. As the organising system thus developed, the time required to find each adaptive response would rapidly diminish, if the number of degrees of freedom were allowed to remain fixed. The suggestion is, however, that as adaptive organisers develop, the system should gradually increase the number of its degrees of freedom, by dissolving selectively (according to success or failure) the statistical links between the controls previously grouped together. The rate of this process of selective differentiation may be controlled, automatically or otherwise, so as to keep the informational efficiency of the trial-and-error process near its maximum.

In this way a large proportion of the information received in experience could be utilised in the structuring of internal organisation, and the rate of development apparent in the human organism could easily be achieved.

5. Some Implications

5.1. Since each feature of the world is represented internally on this theory by the internal organiser of the appropriate 'state of readiness' for that feature, it follows that *thinking* should involve the running of such organisers, but with the suppression of most or all of their normal overt consequences.

In problem-solving, for example, wholly internalised processes of trial and error are possible, since the organising system offers in a sophisticated sense an internal model [1] of the conceptualised world—a field for internal goal-directed activity using largely the same organisers as in responses to the corresponding features of the world.

5.2. Thus although on this theory thinking is an activity of the central rather than the peripheral nervous system, it would be expected on the one hand that thought should be accompanied by peripheral changes (say in the excitation

[1] Not indeed in Craik's (5) sense of a physical analogue, but in the sense of a logical relation-structure.

thresholds of muscles) and on the other, that damage to some organisers of motor activity should affect thinking. The detailed study of aphasia and agnosia may thus provide one of the most direct sources of evidence to enable the present information-flow-model to be developed and tested.

6. Conclusion

Let me end with a more general remark. Information theory, like all novelties, runs a risk of being 'over-sold.' It is fashionable to translate familiar facts into the new terminology, without always gaining in clarity thereby. What then of the information-flow-model of the human organism? Will not it, even if successful, amount only to one more retranslation of the facts that need explanation?

A translation, in one sense, it certainly is. But it differs from others in one fundamental respect. Each re-description of symptoms or performance in information-flow language amounts also to a hypothesis as to their structural basis. Information-flow language is justified only insofar as it serves this purpose of generating fruitful hypotheses from data which would not otherwise suggest them.

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(1) See, for example, the B.A. Symposium on 'Cybernetics,' 1953: *Advanc. Sci.* **40** (March 1954).
(2) *Ibid.* pp. 402–6.
(3) MacKay D. M. (1955): in *Automata Studies*. Princeton Univ. Press.
(4) MacKay, D. M.: 'Towards an Information-Flow Model of Human Behaviour'; *Brit. J. Psychol.* (In press.)
(5) Craik, J. W. (1943): *The Nature of Explanation*. C.U.P.

Behavioural Studies of Cerebral Dysfunction

by

Malcolm Piercy

Neuro-physiology and experimental psychology would seem largely to be interested in the same thing: the mechanism of the behaviour of men and animals. Nevertheless, differences in the methods which they employ result in particular fields of investigation which overlap comparatively little. Thus, physiological accounts of the adjustment of an organism to its environment are closely correlated with anatomical structure and consequently physiology permits itself to give a less comprehensive account of behaviour than psychology is prepared to attempt. Present physiological knowledge of the characteristics of nerve cells and of systems of nerve cells, taken in isolation, would not justify conclusions as to the type of general behaviour of which the organism is capable; i.e. the behaviour studied by the psychologist.

Conversely, experimental psychology analyses behaviour in more general terms and, although it may be able to establish laws governing the behaviour which results from modifying the environment, it very largely neglects the precise nature of the nervous events intervening between stimulus and response. In fact, an analysis of overt behaviour along psychological lines would of itself provide very little information as to the nervous events which make this behaviour possible; i.e. the nervous events studied by the neurophysiologist.

It is, of course, a commonplace of the methodology of the natural sciences that there should be two separate disciplines when each has only a very limited predictive power within the field of enquiry of the other. If, however, there is a body of data which is relevant to both sciences, but which is poorly assimilated into the theoretical framework of either, there is likely to be a need for research which co-ordinates both theoretical approaches. In the case of neuro-physiology and experimental psychology such a problem is presented by the effects of cerebral lesions. The body of information which has been built up by psychology concerning motivation, learning, intelligence, perception and emotion may enable the experimenter to predict (within rather wide limits) how a normal organism will behave in particular situations. However, this empirical knowledge is not of a sufficiently intimate nature for a prediction to be made as to how behaviour would be modified by physical interference with cerebral function. Furthermore, the observed behavioural effects of cerebral dysfunction do not fit in very well with psychological theories based on studies of normal behaviour. Similarly, these behavioural

changes could not be predicted on the basis of knowledge so far acquired through strictly physiological studies and pose very many more problems for neurophysiological theory than they solve.

It seems reasonable to suggest that the careful analysis of the behaviour of organisms whose cerebral function has been modified provides the point of departure for studies which may eventually modify both physiological and psychological theories as to how behaviour is organised.

The following account describes first a group of recent studies of cerebral dysfunction which is of some theoretical interest and then two studies of abnormal behaviour in normal humans which may be co-ordinated with pathological data.

Neurologists have for many years recognised that focal cerebral lesions may produce bizarre defects of specific aspects of intellectual function. Although these syndromes may grossly overlap and vary greatly from one case to another in the pattern of disability which occurs, nevertheless they have identifiable features which suggest a certain functional discreteness in the organisation of some intellectual skills. There are in other words lines of cleavage which determine the type of breakdown which will occur. One of the best known of these focal intellectual breakdowns is the disturbances of language functions generically referred to as aphasia.

It is generally acknowledged that in right-handed people aphasia occurs much more frequently in association with lesions of the left hemisphere than with lesions of the right hemisphere. The assumption was originally made that the converse effect applies to left-handed people ; that if they develop an aphasia it will most frequently be in association with a right hemisphere lesion. However, it is now apparent that aphasia in a left-handed person with a left hemisphere lesion is far from an exceptional observation. The evidence on this point and its theoretical implications were fully discussed by Professor Zangwill at the 1954 meeting of the British Association. Even more recently, Goodglass & Quadfasel (1954) published a study showing that of 103 cases of aphasia with a unilateral lesion involving the so-called language area as many as 53 had a left-sided lesion. So it seems that dominance for language functions by the hemisphere contralateral to the preferred hand is much more frequent in right-handers than in left-handers. Also aphasia tends to be less severe and more transient in left-handed than in right-handed patients. Recovery is perhaps facilitated by an anatomically more diffuse organisation of language functions.

It is of some interest that there are certain similarities between the cerebral organisation of language in left-handed people and that in children. Thus, in children, aphasia following a right hemisphere lesion is more common than it is in adults and recovery from aphasia tends to be more rapid. These differences between child and adult are strikingly similar to those between left-hander and right-hander. It is not surprising that cerebral language mechanisms should be less differentiated in children ; this may also be the case with left-handers. At all events these differences between left- and right-handed people in language organisation appear to confront us with the interesting situation of varying degrees of cerebral specialisation for a particular function in two identifiable groups of people. Does this enable us to elucidate any further the implications of more specialised and less specialised areas of the brain ? A study which is relevant to this point was carried out by Hécaen & Piercy (1955), who investigated the incidence of short-lived attacks of aphasia which occurred during epileptic auras resulting from a unilateral focus of cerebral dysfunction. They found that such paroxysmal aphasia was much more common in left-handed than in right-handed patients suffering from epileptic auras, irrespective of the side of the focal disturbance. This is in contrast to the evidence from permanent aphasia occurring with a chronic cerebral lesion, since it is not usual to suggest that a left hemisphere lesion more frequently results in aphasia in left-handers than in right-handers. An explanation may be sought in the nature of the disturbance which is studied in the two kinds of investigation. The paroxysmal defect was of abrupt onset and probably far too short-lived for any compensatory activity to be organised.

In the case of permanent aphasia, however, the disturbance of language which is observed is presumably the product both of the immediate defect produced by the lesion and any restitution of function which may have occurred from the activity of undamaged cerebral areas. It could be that greater diffuseness of organisation of a function within the brain involves on the one hand the possibility of greater restitution of function when the disturbance is chronic, and on the other hand greater susceptibility to acute short-lived interference. One may draw an analogy and say that provisional systems are more easily disrupted but more readily reorganised than are more closely integrated systems.

Such studies of cerebral dysfunction may profitably be compared with studies which produce abnormal behaviour simply by modifying the environment and without any direct interference with cerebral function.

If a person's speech is picked up by microphone and played back to him amplified through earphones with a delay of about ½ second, his speech is likely to be dramatically impaired. The subject for such an experiment does not hear what he says as he says it but instead hears it ½ second later when he is perhaps one word further on in his sentence. This results in a disorganisation of speech which is not unlike an expressive dysphasia as produced by a cerebral lesion. The environmental change which has produced the experimental disturbance can be defined fairly precisely : it is a shift in the time relations between vocalisation and auditory check-up (or feed-back) of what has been said. Further studies of this kind could conceivably help to define in operational terms what it is that goes wrong when a patient becomes aphasic.

Another experiment which produced abnormal behaviour in normal people was recently carried out in Canada (Hebb, 1955). It was an attempt to isolate a man from his normal environment. Adult volunteers were paid to do nothing and to hear and see nothing 24 hours a day. The subject lies on a comfortable bed, wears frosted goggles and his ears are covered by a sponge rubber pillow. The hands are gloved and a cardboard cuff prevents them from touching things. Under these conditions the subject complains that he cannot concentrate and in fact becomes less efficient at solving problems. On emergence from this isolation his performance on intelligence tests is significantly worse than it was prior to the experiment. During the experiment visual hallucinations were frequent and vivid. There were also feelings of depersonalisation and other bizarre impressions which the subjects found difficult to describe. One subject is reported to have said that his mind was like a ball of cotton wool floating in the air above him. These bizarre symptoms are strongly reminiscent of the effects of hallucinogenic drugs such as mescaline and lysergic acid, but are produced simply by reducing sensory stimulation.

Here there is similar symptomatology resulting from radically different experimental procedures. This may or may not mean that a common cerebral mechanism is being interfered with, but such points of correspondence must at least provide stimulus for further research. Indeed it is to be hoped that the convergence of different scientific methods will yield an improved theoretical structure to the problems of cerebral function. It may be too much to hope that all psychological problems of behaviour will eventually be completely underpinned by physiological knowledge, but the theoretical development of the two disciplines may nevertheless profitably keep step, preferably by discovery in each providing theoretical correction for the other.

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RESPIRATION[1]

by

A. N. GLAZER

Endeavour Prize Essay

OUR present concept of respiratory activity is the culmination of over two hundred years' research into the chemical nature of respiratory processes and of developments in the search for the site of respiratory activity within the living organism.

It is a matter of common experience that life is intimately associated with the process of breathing. The cessation of breathing, which is a concomitant of death, provoked much speculation on the part of Greek and Egyptian philosophers. Their explanations of respiration ranged from that of Aristotle, who stated that breathing was the form of refrigeration by means of which warm-blooded animals 'tempered their inner heat,' to that of Herophilus and Erasistratus, the leaders of the Alexandrian School (*c.* 300 B.C.), who, with greater physiological insight, said that the respiratory movements of the lungs permitted the conduction of a vivifying pneuma through the arteries.

With the advent of Galen (A.D. 131–203) came the first hazy idea that there was some process of a chemical nature involved in respiration. Galen postulated the absorption of a vital spirit in the pulmonary diastole and the expulsion of a mixture of water vapour and a fuliginous vapour in the systole. Owing to the great reverence in which Galen's work was held no attempt was made to dispute it in any way during the Middle Ages.

With the development of the experimental method, a burst of activity in the seventeenth century ushered in an era of rapid development in the physiological and chemical sciences. The problem of the site of respiration in the body attracted many investigators.

The first definite statement as to the site of respiration was made by Robert Hooke (1667). Hooke's famous experiment, upon the results of which this statement depended, consisted of blowing air rhythmically from a pair of bellows into the lungs of a dog whose thorax had been opened. He found that the animal died only when the ventilation of the lungs was discontinued. He concluded, therefore, that the aim of respiration was the renewal of the pulmonary air, and that the movement of the lungs was not, as had been believed by some, essential for the maintenance of life.

The next major step was taken by John Mayow (1640–79). Mayow, whose views coincided with the iatro-chemical theories of vitalism, held that air was composed of a mixture of at least two gases. He stated that chemical change in general derived from the interaction of the combustible component of the air, which he termed 'spiritus nitroaerus,' and sulphur. He contended that during respiration the nitro-aerial spirit entered the blood, making it capable of producing heat. His views focused the attention on blood as the site of respiration.

The work of Black on 'fixed air,' followed by Lavoisier's brilliant work on oxygen and oxidation, and the latter's correct assessment of the nature of respiration, as shown by his conclusion that 'respiration is a slow combustion of carbon and hydrogen similar to that which occurs in a burning candle,' led to the complete revision of pre-existing conceptions. In the matter of the site of respiration, Lavoisier formed the faulty opinion that combustion took place in the lungs. He claimed that in the lungs the oxygen came in contact with a hydrocarbonous

[1] Essay awarded the first prize in the 1955 competition sponsored jointly by the publishers of *Endeavour* and by the British Association. Mr. Glazer is an Australian and is a student at the University of Sydney, New South Wales.

fluid, a view which was not refuted until the experiments on frogs and fish carried out by Edwards in 1824.

The concept originating in the posthumously-published work of Abbé Spallanzani (*Rapport de l'air avec les êtres organisés*, Geneva, 1807) that the tissues were the seat of respiratory activity was further developed in 1872 by Lothar Meyer, Ludwig and Pflüger. These investigators demonstrated by means of a mercurial air-pump that oxygen was taken up in the lungs and subsequently released in the systemic circulation owing to the lower partial pressure caused by the tissue consumption of oxygen. The concept of tissue respiration was, however, not accepted universally until as late as 1880.

Kingsbury pioneered the next major advance when, in 1912, he stated that mitochondria were concerned in respiratory activity. The association of the mitochondria with respiration was not unexpected, as the development of cytology had shown these organelles to be almost omnipresent in living tissue.

Mayer, Rathery and Schaeffer (1914) supported this view on the basis of the phosphatidal content of mitochondria—an attribute which made these organelles well adapted for oxidation-reduction reactions.

Before going on to discuss the recent developments in the study of the organisation within the mitochondria, the parallel development in the elucidation of the chemical mechanisms involved in respiration requires review.

It was clear that oxygen in its molecular state had no effect on those substances, which it could in some fashion oxidise within the tissues. Schönbein's discovery of ozone in 1840 began the work on the problem of activated oxygen. Schönbein suggested that oxygen existed in three allotropic forms: neutral o, ozone ⊖, antozone ⊕. He held that in oxidation oxygen splits into ozone and antozone, the ozone being used for the oxidation of the metabolite, while the antozone is the 'active' form of oxygen.

Hoppe-Seyler modified this view when in 1878 he suggested the following mechanism of activation:

$$AH_2 + O{-}O = A + H_2O + {}'O'\ \text{active}$$

As an example of this type of reaction, he took the following system:

$$PdH_2 + O_2 + \text{Indigo} = Pd + H_2O + \text{oxidised indigo}$$

A well-known experimental fact was that in oxidations of phosphorus, benzaldehyde and other substances a lower oxide and a peroxide are formed in equivalent proportions. On the basis of this observation, Bach (1897) and Engler and Wild (1897) independently suggested that organic peroxides were formed which had the power to oxidise other compounds:

$$A + O_2 = A\begin{matrix}O\\|\\O\end{matrix}$$

$$A\begin{matrix}O\\|\\O\end{matrix} + A' = AO + A'O$$

Despite the extremely slender foundation upon which this theory rested it was not displaced for almost twenty years. On the contrary, it was adopted and supported by some of the most eminent scientists of the day.

In 1903, Bach and Chodat prepared a solution from horse-radish roots; this solution did not give off oxygen gas nor did it produce a blue colour with guaiacum. When hydrogen peroxide was added to this solution, guaiacum was oxidised with the production of the blue colour.

On the basis of this and similar experiments, Bach and Chodat extended the organic peroxide theory. They resolved the positive guaiacum test into two parts:

They concluded that an 'oxygenase' formed the organic peroxide in the air, the peroxide being subsequently broken down to give water and active oxygen which oxidised the guaiacum giving the blue reaction.

$$A + O_2 \xrightarrow{\text{Oxygenase}} A\begin{matrix}O\\|\\O\end{matrix}$$

$$A\begin{matrix}O\\|\\O\end{matrix} + A' \xrightarrow{\text{Peroxidase}} AO + A'O$$

Warburg (1918) found that a suspension of blood charcoal in water was capable of

utilising molecular oxygen for the oxidation of substances such as oxalic acid. On investigating further, he found that the behaviour of blood-charcoal systems was similar in many ways to that of living cells. Thus narcotics such as urethane and inhibitors such as cyanide had a comparable effect on the respiration of living cells and of charcoal. As pure charcoal obtained from sucrose did not possess the catalytic properties of blood-charcoal, Warburg concluded that the catalytic properties of the latter were connected with its iron content. He accumulated a great deal of experimental evidence in support of his conclusion.

The autoxidation of cysteine was carefully studied. On exposure to air cysteine was found to be slowly oxidised to cystine in the presence of catalytic amounts of iron impurities. When these were removed, molecular oxygen could no longer convert cysteine to cystine.

Warburg also found that carbon monoxide inhibited the respiration of cells in the dark, but that the inhibition was reversed by light. It was known that carbon monoxide forms complexes with several heavy metals. Its complex with iron was known to be photolabile.

On plotting the absorption spectrum of the carbon monoxide compound of the respiratory enzyme, Warburg found that the curve obtained closely resembled that obtained from the spectrum of a carbon monoxide-haemochromogen. He concluded from his experiments that ' the primary reaction of respiration is the reaction between molecular oxygen and iron, and molecular oxygen can only react in the cell by this reaction, and not with the organic molecules.'

Warburg believed that the entire uptake of the cell was funnelled through one catalytic enzyme—the ' respiratory enzyme.' His theory of oxidation of organic molecules, which resembles that of Bach, may be formulated as follows :

$XFeO_2 + 2A = XFe + 2AO$, where XFe is the ' respiratory enzyme '.

Although many criticisms were levelled at Warburg's theory—for example, that the respiration of most tissues is inhibited to the extent of only 40–60 per cent. by cyanide, the existence of his ' respiratory enzyme ' and its importance were undisputed. The criticisms only served to emphasise the fact that there were other mechanisms present, which were capable of maintaining a portion of the respiratory activity.

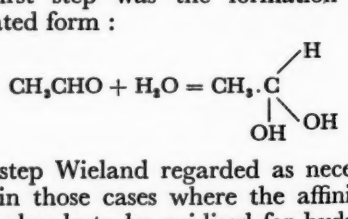
It was realised early that the oxidation of organic molecules by the addition of oxygen to the molecule concerned gave the same result as the removal of hydrogen from that molecule or its hydrate.

The view that the essential mechanism is the activation and removal of hydrogen was first put forward by Wieland in 1912. Wieland's ideas were based on a series of experiments carried out using reagents rarely, or never, found in the normal cell, and on this score were open to serious objections.

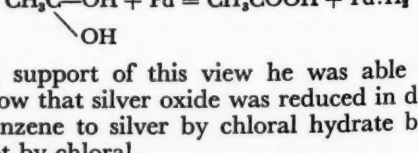
Wieland drew attention to the fact that many organic compounds could be oxidised by the addition of colloidal palladium to their aqueous solution. In the process of the oxidation, the palladium became ' charged ' with hydrogen. Wieland found that the ' charged ' palladium was able to pass on its hydrogen to reducible substances such as methylene blue.

From the above observations, Wieland suggested a mechanism of oxidation which is well illustrated by the conversion of acetaldehyde to acetic acid.

According to the Wieland mechanism, the first step was the formation of a hydrated form :



This step Wieland regarded as necessary only in those cases where the affinity of the molecule to be oxidised for hydrogen had to be reduced, enabling the latter to pass to the palladium in the second step :



In support of this view he was able to show that silver oxide was reduced in dry benzene to silver by chloral hydrate but not by chloral.

The vindication of Wieland's views came in 1920, with Thunberg's invention of the both elegant and simple technique

of the evacuated tube. Experimenting on chopped frog muscle, and using methylene blue as the hydrogen acceptor, Thunberg found that chopped frog muscle was able to reduce the dye rapidly, but that this attribute was lost on washing. If, however, substances such as sodium succinate or lactate were added to the washed muscle, rapid reduction of the dye took place. If the frog's muscle is substituted for palladium in Wieland's scheme, it is obvious that the two situations are identical.

Thunberg's technique was extremely effective. By its means, he was able to demonstrate the presence of, in frog muscle, the following dehydrogenases: lactic, oxybutyric, succinic malic, citric, tartaric, glutamic and glycerophosphoric. The full value of his method can only be assessed if the later discovery of many dehydrogenases through it be taken into account.

In 1884–6 MacMunn, during his investigations into the absorption spectra of various tissues, observed absorption bands which evidently belonged to substances of the haematin group. He gave the names of myohaematin and histohaematin to the substances, to which he attributed the generation of the observed bands. His work fell into disrepute following sharp criticism by Hoppe-Seyler and other workers in the field. These claimed that MacMunn's myo- and histohaematin were merely decomposition products of haemoglobin. MacMunn pointed out that the spectral lines generated by the haemochromogen obtained from haemoglobin did not exactly coincide with those he had observed. He further argued that these newly found absorption bands could be detected in many places; in insects, for example, where haemoglobin is not found. Very little attention was paid to his work, which lay almost forgotten for forty years.

In 1925 Keilin found bands like those of MacMunn in every aerobic cell he examined. He renamed the substance generating these bands 'cytochrome.' A relation between the respiratory activity of a tissue and its cytochrome content was soon noticed.

Keilin concluded from his examination of the spectral bands that cytochrome could be resolved into three separate haemochromogens, a, b and c. In 1930 Keilin and Hill succeeded in extracting the 'c' component of cytochrome from baker's yeast. They found that it was readily reduced by mild reducing agents, but once reduced it could not be reoxidised merely by shaking with oxygen. In cell suspensions, however, it was readily reoxidised on admission of oxygen. From these experiments it was concluded that an enzyme, cytochrome oxidase, is present in the cell for the purpose of reoxidising reduced cytochrome.

On further investigation, it was shown that N/10,000 potassium cyanide will stop the oxidation of cytochrome. On the other hand, the narcotics, such as urethane, which were found to inhibit Thunberg's dehydrogenase systems, did not affect the oxidation of cytochrome. They did, however, stop its reduction. In a urethane-treated yeast cell, oxidised cytochrome was not reduced by the addition of cyanide, this showing that the reducing ability of the cells was impaired.

From the above observations it was concluded that cytochrome was an intermediate carrier between the dehydrogenase systems and the systems which oxidise it.

Keilin (1929) demonstrated that the catalyst which brought about the oxidation of cytochrome by atmospheric oxygen was identical with a widely distributed enzyme—indophenol oxidase. He showed that this enzyme was inhibited by all those agents which inhibit the oxidation of cytochrome. He further showed that indophenol oxidase formed a photolabile complex with carbon monoxide. From these and more detailed studies of the carbon monoxide complex, it became clear that Warburg's 'respiratory enzyme' was in fact indophenol or cytochrome oxidase.

The work of Keilin, following closely, as it did, upon that of Wieland, Thunberg and Warburg, completed the framework upon which all of the subsequent developments in this field have rested. Indeed, it could be said that the concepts held today are only highly detailed and elaborated forms of those put forward by Wieland, Warburg and Keilin.

The picture which the co-ordinated work of these scientists presented was in essence this: the substrate, 'food-stuff' molecule, is dehydrogenated by an enzyme

specific for that substrate ; the enzyme is reoxidised by a hydrogen acceptor (represented *in vitro* by a dye such as methylene blue), then, through the intervention of some unknown factor, the electrons from the removed hydrogen atoms are passed along the cytochrome system, finally reducing Warburg's enzyme, presumably cytochrome oxidase, being thence passed on to molecular oxygen.

The work done on the factors involved and the mechanisms of reaction in tissue respiration from about 1925 to the present day has consisted of a multitude of advances, some of great importance, some which merely represented an improvement of an already existing technique or the elaboration of a new one. Taken together these advances have been illuminative enough to fill most of the gaps in our knowledge of tissue respiration.

Simultaneous advances were taking place in the work on the different aspects of dehydrogenase action and electron transport as well as in the study of the functions of vitamins and trace elements. The continual interweaving of these lines of development has led to an increasingly detailed picture of the metabolism as a whole.

It had often been observed that many insoluble dehydrogenases lose their power to catalyse the reduction of methylene blue after prolonged dialysis. If, however, some boiled tissue juice, or the diffusible fraction of boiled yeast juice, were added, the enzymes recovered their activity.

Szent-Gyorgyi (1925) succeeded in the isolation of a thermostable, dialysable compound which contained adenylic acid, and functioned as a stimulating factor, or a coenzyme, to the lactic dehydrogenase of heart muscle.

A short time later, Warburg and Christian succeeded in isolating a similar compound, the coenzyme of hexose monophosphate dehydrogenase. Warburg, Euler, and their collaborators established the structures of these two coenzymes. Szent-Gyorgyi's coenzyme was shown to have the structure :

D-ribofuranoside—nicotinamide
|
phosphate
|
phosphate
|
D-ribofuranoside—adenine

and Warburg and Christian's coenzyme was shown to have a structure differing from the above by an additional phosphate group, whose exact position is even today disputed. The two coenzymes were named coenzyme I (or diphosphopyridine nucleotide, DPN) and coenzyme II (or triphosphopyridine nucleotide, TPN) respectively.

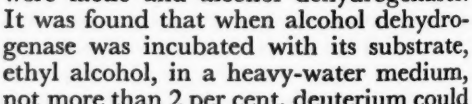
The part of these large molecules which has been shown to be involved in the oxidation-reduction reaction in the nicotinamide radical. One of the phosphate groups is present in the ionised form. Karrer has shown that the oxidation-reduction equation for the pyridine nucleotides may be written as :

$$DPN + 2H^+ + 2e = DPNH + H^+$$

One hydrogen and two electrons add on to the pyridine ring, while the remaining hydrogen ion is neutralised by the negatively charged ionised phosphate group. At one time it was believed that the hydrogen added on in the 2 position, but recent evidence has shown that the reduction probably takes place at the 4 position of the pyridine ring (1).

Recent experiments (2), utilising heavy water, have thrown some light on the interaction between the dehydrogenase, its specific pyridine nucleotide coenzyme and the substrate. The enzymes studied were lactic and alcohol dehydrogenases. It was found that when alcohol dehydrogenase was incubated with its substrate, ethyl alcohol, in a heavy-water medium, not more than 2 per cent. deuterium could be found in the reduced coenzyme I, while if dideuteroethanol was used in a water medium, the reduced coenzyme I contained deuterium only. This and similar experiments carried out using lactic dehydrogenase showed conclusively that the extra hydrogen atom which appears in the enzymatically reduced coenzyme I does, in fact, come from the substrate, no exchange with the medium taking place.

Steric configuration of monodeutero reduced CoI permits two diastereoisomers:



Yet, in the enzymatic oxidation of reduced deutero-coenzyme I, DPND, all

the deuterium is removed from the coenzyme. If chemically reduced coenzyme I is used, only just over half the deuterium is removed.

This almost absolute preference for deuterium over hydrogen in the case of enzymatically reduced coenzyme I conclusively shows that this coenzyme consists of only one of the two above-mentioned diastereoisomers.

This demonstration of the stereochemical specificity of the pyridine nucleotides, coupled with the almost complete lack of exchange of the reducing hydrogen with the medium hydrogen, suggests that the hydrogen atom probably passes straight from the substrate to the coenzyme while they are both brought into close juxtaposition at the active centre of the enzyme.

The formation of enzyme-coenzyme compounds has been studied recently spectroscopically in the cases of mammalian triose phosphate dehydrogenase-DPN, alcohol dehydrogenase-DPNH, and lactic dehydrogenase - DPNH. Although the studies prove the formation of such compounds, the nature of the bond between the enzyme and coenzyme still remains a matter for speculation.

The great specificity which the dehydrogenases show towards their substrates is mirrored in their specificity towards the first hydrogen acceptor. With the isolation and purification of the coenzymes it has become possible to divide the anaerobic dehydrogenases into three classes—those which are DPN specific, those which are TPN specific, and those which require no coenzyme.

The work of Adler *et al.* (Stockholm, 1937) and Green *et al.* (Cambridge, 1937) led to the conclusion that a factor is necessary for the transport of electrons from reduced coenzyme I to the cytochrome system. These workers had shown that DPN-specific dehydrogenases were unable to react directly with cytochrome c, methylene blue or atmospheric oxygen.

By this time, considerable data had accumulated on the catalytic properties of flavoproteins. The question of flavoproteins as intermediates in biological oxidation first came to the fore with the publication by Warburg and Christian (1932) of a paper on the oxidation of glucose-6-phosphate by mammalian erythrocytes. These workers showed that for the oxidation of glucose-6-phosphate by atmospheric oxygen or, under anaerobic conditions, by methylene blue, a material present in the erythrocyte was required. This material was found to be present in abundance in yeast, and was thence isolated in the form of a yellow protein-bound pigment. This conjugated protein was named ' the yellow enzyme.' Subsequent work, carried out mainly by Theorell, showed that the pigment was riboflavin-5′-phosphate. The flavin-free enzyme was found to be ineffective as a catalyst in the oxidation of glucose-6-phosphate by oxygen or methylene blue, but the addition of the riboflavin-5′-phosphate restored the catalytic activity.

Another flavoprotein, having flavin-adenine dinucleotide as a prosthetic group, was isolated by Krebs in 1933. This enzyme was found to catalyse the oxidation of D-amino acids with atmospheric oxygen.

When the matter of an intermediate between the DPN-dehydrogenases and cytochrome was raised by Adler and Green, attention was immediately directed to the flavins. It was found, however, that the rate of interaction of DPNH and TPNH with the prosthetic groups FP and FAD of suitable flavoproteins and the rate of oxidation of these by molecular oxygen, were so low that this system could only be regarded as accounting for a negligible fraction, if any at all, of the observable respiration.

The flavoproteins assumed a new importance when Haas *et al.* (1942) isolated and purified a new flavoprotein from yeast, which they named cytochrome c reductase.

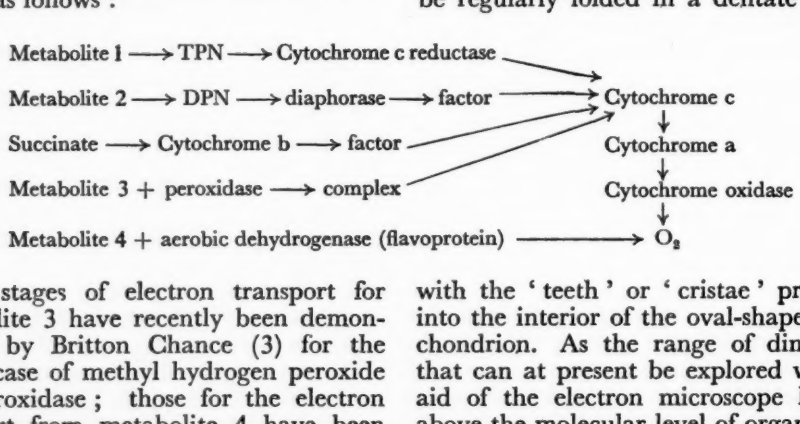
Owing to its great ease of denaturation this protein had escaped the notice of earlier investigators. While the ' yellow enzymes ' isolated by Warburg and others reacted but slowly with cytochrome c, the new Haas enzyme reduced cytochrome c fourteen times as fast as the ' old yellow enzyme ' when TPNH was used as the electron donor. The prosthetic group of this enzyme was shown to be riboflavin-5′-phosphate. With DPNH as the electron donor the enzyme reacted very slowly.

In view of these results it became clear that a pathway exists from TPNH to cytochrome c via the flavins as electron

carriers. With regard to DPNH the position is not so clear. Although the intermediate carrier here has not been isolated in the pure state, certain enzyme preparations from swine heart and liver show considerable activity. There is another flavoprotein, having FAD as a prosthetic group, which is capable of catalysing the reduction of FAD by DPNH. The name ' Diaphorase ' has been assigned to this enzyme. The reduction of FAD by DPNH proceeds at a reasonable rate, but the reoxidation of the FAD is a rather slow process. It is difficult to assess the importance of this system, especially as evidence has been offered that an unidentified oxidation reduction system is interposed between diaphorase and cytochrome c.

The dehydrogenases not requiring either DPN or TPN, such as, for example, the well known succinic dehydrogenase, have been shown to join the main transport sequence through the intervention of cytochrome b.

The sequence of electron transfer in tissues may be diagrammatically represented as follows :


Metabolite 1 → TPN → Cytochrome c reductase
Metabolite 2 → DPN → diaphorase → factor
Succinate → Cytochrome b → factor
Metabolite 3 + peroxidase → complex
Cytochrome c
Cytochrome a
Cytochrome oxidase
Metabolite 4 + aerobic dehydrogenase (flavoprotein) → O_2

The stages of electron transport for metabolite 3 have recently been demonstrated by Britton Chance (3) for the model case of methyl hydrogen peroxide and peroxidase ; those for the electron transport from metabolite 4 have been demonstrated in the case of xanthine oxidase and the amino acid oxidases.

The foregoing diagram serves to underline the important fact that, regardless of source, the electrons transferred in the hundreds of different dehydrogenation reactions going on in the living cell follow the same main path to molecular oxygen.

Even such complicated reactions as the oxidation of alpha-keto-glutarate to succinate have been shown to proceed along a pathway involving DPN.

The introduction by Bensley and Hoerr in 1934 of new means of separating out mitochondria from the other cell components permitted the close examination of these inclusions by chemical and physical techniques. Typically the mitochondria have a length of two microns and a diameter of half a micron. It has been concluded by inference from permeability studies that the mitochondrion is surrounded by a membrane possessing osmotic properties. Recent advances in embedding, sectioning and fixation techniques have made possible the study of the mitochondrial structure under conditions more favourable than those hitherto employed in the study of the isolated organelles. This close study has served to emphasise the great organisational unity within all mitochondria so far examined irrespective of cell-type or animal species from which they come.

Some elegant electron - microscope studies (4) have shown that the mitochondrion may be considered as consisting of an outer membrane and an inner membrane. The inner membrane appears to be regularly folded in a dentate pattern with the ' teeth ' or ' cristae ' projecting into the interior of the oval-shaped mitochondrion. As the range of dimensions that can at present be explored with the aid of the electron microscope lies just above the molecular level of organisation, the view is held that the postulated enzyme chains, controlling oxidation and phosphorylation reactions, are disposed along these cristae and, in fact, that the cristae are structures built up of the protein enzymes themselves bound together with phospholipids. This view is supported by the great stability of the cristae as compared with that of the often-broken limiting membrane. Also, particles of the general dimensions of the cristae have been isolated from suspensions of disintegrated

mitochondria. These fragments have been shown to contain most of the succinoxidase system of the cell.

The evidence for the presence of these enzymes systems in the mitochondria has been accumulating steadily over the past ten years. The earliest work which may be considered to be of value in this matter is that of Schneider (1946) (5). All the work done prior to that of Schneider is open to some criticism on the grounds that no effort had been made to determine whether the total enzymatic activity of the various fractions obtained by the differential centrifugation of the disrupted cell amounted to the activity observed in the intact tissue.

In 1946 Schneider separated rat liver and kidney into a nuclear fraction, a large granule fraction and into a residue which was not fractionated further. He then proceeded to study the distribution of succinoxidase, cytochrome oxidase and adenosine triphosphatase in these fractions. The reliability of his technique was borne out by the fact that the sum total of the enzyme activities of the fractions was equal to that of the original suspension. His work resulted in the finding that succinoxidase, cytochrome oxidase and ATP-ase were found mainly in association with the large granule fraction. In the same year Hogeboom, Claude and Hotchkiss (1946) (6) reported that almost all of the succinoxidase and cytochrome oxidase activity of cell-free rat liver extracts was associated with the large granules.

In 1947 Pallade improved the technique for the isolation of mitochondria and presented strong cytological evidence in support of the view that the isolated particles were mitochondria. These particles were found to behave in the same way as the large granule fraction studied in 1946.

As a result of a great deal of research, it has been shown that all mitochondria, regardless of source, carry out three fundamental sequences, the citric acid cycle and ancillary processes, oxidative phosphorylation, and the transfer of electrons from substrate to oxygen. With regard to other sequences, such as fatty acid oxidation, the utility of the mitochondria varies depending on their source. The mitochondria, when properly prepared, have been shown to contain the full complement of enzymes and coenzymes necessary for maintaining the processes which they catalyse.

When the mitochondrial size is considered on the molecular basis it may easily be seen that these organelles are excellently suited for their tasks. Thus Tracey (7) has calculated that if the protein content of a single mitochondrion was half of its dry weight, 1,000,000 protein molecules of the average size could be contained within it. This would be enough for 2,000 duplicates of 25 different enzyme systems, each composed of 20 enzymes.

While it is universally recognised that the purpose of respiration is the generation of energy-rich bonds, a definite tendency exists to discuss oxidative electron transfer and oxidative phosphorylation in such a way as to give the impression that the latter is merely an attendant circumstance rather than the actual purpose of the first, which it undoubtedly is.

It would not be too far-fetched to assume that since one-step pathways of oxidation exist (e.g. oxidation of xanthine by xanthine oxidase), the extremely complex system involving the pyridine nucleotides, the flavins and the cytochromes would have functions other than that of electron transport alone.

The fact that in the mitochondria two of the three sequences, invariably found together, are phosphorylation and electron transfer mechanism, also assumes some significance in this context.

Further, the change in free energy on passing along the chain of electron carriers is large enough to permit generation of energy-rich phosphate bonds at every step.

In 1939, the Russian workers, Belitzer and Tsibakova (8), had reported that in minced heart and skeletal muscle preparations nearly two moles of phosphocreatine, the storage form of energy-rich phosphate bonds in mammalian tissues, were formed per atom of oxygen consumed. This observation implied more than one phosphorylation per pair of electrons passed from substrate to oxygen. As an explanation, these workers proposed that intermediate steps in the electron transfer, as well as those involved in substrate oxidation, must be responsible for phosphorylation.

In 1941, Ochoa (9) demonstrated that the P : O ratio (moles of inorganic phosphate converted to the organic form : atoms of oxygen consumed) approached 2·0 for the oxidation of pyruvate (later corrected to 3·0) when glucose was used as phosphate acceptor, and was in general greater than 1·0 in cell-free preparation.

The work of Ochoa, Belitzer and Tsibakova forcibly showed that it was necessary to look for mechanisms of phosphorylation, not, as had hitherto been thought, among the phosphorylated oxidation products of the substrates only, but also in the electron transport chains.

The first important success in the search for mechanisms coupling phosphorylation to the carriers involved in electron transport came in 1949, when Lehninger (10) succeeded in demonstrating that the oxidation of DPNH by oxygen gave rise to phosphorylation. Lehninger achieved this result by preparing highly purified DPNH and determining the conditions most suitable for its oxidation. He observed directly a P : O ratio of 1·5 (observed as ratio of inorganic phosphate removed : DPNH disappearing).

When dinitrophenol is added to such a preparation, the respiration is stimulated. It is known that dinitrophenol uncouples oxidative phosphorylation. It is thus possible that phosphorylated intermediates are formed in oxidative phosphorylation with the pyridine nucleotide, which saturate the electron carrier system at this point; recent evidence suggests that the addition of dinitrophenol by stimulating the breakdown of these intermediates promotes the free flow of electrons to molecular oxygen by way of the liberated carriers, thus increasing the respiration.

Hummel and Lindberg (1949) (11) and Good and Leaf (1950) (12), working with radioactive phosphorus, have demonstrated that FAD participates to some extent in phosphorylation. However, no mechanism has been suggested in this case.

As no success has been obtained with cytochrome c, as far as oxidative phosphorylation is concerned, it is thought that this system is probably extremely labile and hence very difficult to detect.

In general, it can be said that the manner in which the electron-transfer chain is linked to phosphorylation is still largely a matter of speculation.

At the present state of our knowledge, it has become clear that the respiratory channel of electron carriers, which is the terminal stage of most catabolic reactions involving 'foodstuff' molecules, is, in fact, the energy supply source for the cell processes.

The study of the high structural organisation within the mitochondria has provided us with an explanation of the great efficiency of the enzyme chains within the living cell.

We have still, however, before us the great problem posed by the mechanism of generation of energy-rich phosphate bonds —the problem which has so far withstood the onslaught of the near-perfect modern methods of research.

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TAKING THE NUCLEIC ACID JACKETS OFF BACTERIA[1]

by

PROFESSOR M. STACEY, F.R.S.

RIBONUCLEIC acid (RNA) and deoxyribonucleic acid (DNA) form a high proportion of the organic matter of all bacteria and other micro-organisms. In some yeasts the nucleic acid components may amount to 20 per cent. of the dry weight of the cells. While it is quite evident that DNA plays a vital role in cell division constituting, with combined protein, the major part of cell genes and chromosomes it is by no means clear what are the major functions of RNA. Evidence is growing that it plays an important part in protein synthesis and it also seems to be closely associated with polysaccharide synthesis. At one time it was tempting to think of RNA, which is located mainly in the cell cytoplasm, as a precursor of the DNA of the nuclear material but there is very little evidence for this.

In this article it is proposed to deal with the significance of both types of nucleic acid on or near the surface of some bacteria. Our particular concern has been to find what connection they have with complex polysaccharides which are well known to be surface or ' capsular ' components of the bacterial cell.

Mankind has waged a war throughout the ages against parasitic bacteria which are pathogenic to animal and human bodies. Although we have conquered most of the infectious diseases with measures such as Public Health systems, antibiotics, chemotherapeutics and vaccines we need to be continually alert. We still do not know precisely how our vaccines act and we know very little about the way in which chemotherapeutic agents and antibiotics affect selectively the bacterial cells. We know that a lot of chemistry is involved and our idea had been to learn something of chemical nature of the bacterial surface—the outer defences of the invading parasite.

There are, of course, enormous differences between bacteria and each eventually will have to be treated from the chemical point of view as an individual. Our bacteriological colleagues have built up a vast store of knowledge regarding the habits of all these individuals. By microscopic, staining, fermentation and immunological techniques it is possible to identify them all rapidly and with great precision. The chemical reasons for the characteristics of morphology and of staining reactions are but little understood. One in particular is the Gram stain, the most commonly used of all bacteriological staining techniques. It consists, as discovered by Christian Gram, a Dane, in 1883, in staining fixed smears with an aqueous solution of a dye of the *p*-rosaniline series and mordanting with aqueous iodine. Subsequent washing with alcohol or acetone removes the dye from so-called ' Gram-negatives ' and leaves the ' Gram-positives ' stained deeply blue-black. Animal and vegetable cells are not affected by the Gram stain. There are many important microbiological differences between Gram-positives and Gram-negatives. Thus Gram-positives such as the *Pneumococci* usually can be knocked out by chemotherapeutics and antibiotics but give on injection or after infection a poor immunity. Gram-negatives such as the dysentery bacillus, are much less susceptible to chemotherapeutic agents and antibiotics but can give rise to a high degree of immunity.

It seemed apparent that there must be considerable differences between the surface components of Gram-negatives and Gram-positives. Many theories had been

[1] Address delivered to Section B (Chemistry) on September 1, 1955, during the Bristol Meeting of the British Association.

put forward in the past to explain the Gram reaction and most of the early ones were based on studies of the diffusion of the dye through the cell membrane and its subsequent failure to pass out again. Many organisms can readily lose their Gram-positive character under a variety of conditions such as ' old age,' treatment with alkalies and acids, and during the process known as autolysis. The resultant Gram-negative cells usually appear smaller and shrunken and there is an apparent loss of surface material which gives in some cases a reaction for nucleic acids.

With the late Dr. Henry we showed that suitable Gram-positive bacteria could be ' stripped ' of an essential component of their Gram-positive material by extraction with 2 per cent. sodium cholate at 60° and thereby rendered Gram-negative. A fraction obtained from material in the extract, itself Gram-negative, could be ' replated ' on to the Gram-negative ' cytoskeletons ' under reducing conditions to restore in a large measure the Gram-positive characteristics.

This essential part of the extract was identified as the magnesium salt of ribonucleic acid and we expressed the view that the dye-retaining material, i.e. the ' Gram complex ' was a magnesium ribonucleoprotein. This was eventually isolated from a number of Gram-positives by a short autolytic procedure and dissociated into Gram-negative magnesium ribonucleic acid and a Gram-negative basic protein which if the —S—S— groups in the latter were kept intact would recombine to give a Gram-positive mass.

Further support came from various workers who showed that ribonucleases—RNA splitting enzymes—would destroy the Gram complex. Striking effects on Gram-positive cells can be obtained with reagents which have a strong affinity for ribonucleic acid. Lithium salts, for instance, will cause the nucleic acids to aggregate into masses at the sides and ends of rod-shaped bacteria. Surface active agents also cause the RNA to clump initially and then to disappear. The significance of both magnesium and RNA can be demonstrated with one organism, *Clostridium sporogenes*, a Gram-positive, which will grow in Gram-negative form under adverse cultural conditions such as magnesium starvation. Addition of magnesium salts will slowly restore the Gram-positive state while addition of magnesium —RNA will rapidly restore both living and dead organisms to the Gram-positive state.

In removing the surface layers of Gram-positives many other macromolecules are removed, e.g. polysaccharides, fats, phosphatides, etc., but only RNA will ' replate ' in any measure. The replating can be seen in a striking manner by photographs in the ultra-violet. P. Mitchell has claimed that the essential part of Gram complex is a ' P ' factor which is a phosphatide. In our hands, however, extraction, e.g. of *Cl. Welchii* cells with boiling methanol and ether for several hours which would remove all of Mitchell's factor, leaves 80 per cent. of the cells Gram-positive.

As a result of our work we have been able to learn something of the significance of magnesium in Gram-positives. In true Gram-negatives, which incidentally do have a significant RNA content, and in Gram-negative cytoskeletons the magnesium is all in ionic form, while in Gram-positives, the major proportion of it is in co-ordinated form. It thus forms the buttons for the jacket !

My colleagues have shown that this magnesium plays an important role in cell division and is an essential element for most Gram-positives which require a higher proportion of magnesium than do Gram-negatives.

We have not been able to ' plate ' magnesium ribonucleic acid on to true Gram-negatives and we consider that they possess proteins located near the surface which are in quite a different class from those in Gram-positives.

It has been possible on the other hand to use RNA from Gram-negatives and from many other sources to ' replate ' Gram-negative cytoskeletons so that RNA does not show any specificity in this type of interaction with protein. In no case could DNA be plated on to cytoskeletons. Recently we have made many efforts to obtain nucleic acids from yeast by means of mechanical disintegration of cells and we have noted in such cases that the cytoskeletons cannot be restored to the Gram-positive state, neither can cytoskeletons rendered Gram-negative by autolysis methods. The latter may be due to the

very powerful and persistent action of ribonuclease and only quite recently have we been able to isolate nucleoproteins from yeast free of this enzyme.

We have been able to discover that the useful antiseptic detergent, cetyltrimethyl ammonium bromide (Cetavlon) exerts its powerful action on micro-organisms by combining strongly with the surface RNA. It puts a thick blanket round them and the knowledge gained from this study has given us a reagent for separating RNA and DNA. The protein of the Gram complex is highly complex and contains eighteen amino acids. We have shown that there is a close association between the RNA-protein surface components of the Gram complex and polysaccharide synthesis in some micro-organisms. All micro-organisms produce complex carbohydrates during some stage of their growth. In some these form structural material, in others they represent a means of storing energy while in many they form a protective outer envelope termed a capsule. This capsule is particularly prominent with the cocci such as the *Streptococci* and the *Pneumococci* and colonies of the encapsulated organisms are said to be ' smooth.' Given the correct cultural conditions and the right glucoside from which to build, some bacteria can produce relatively enormous quantities of capsular material and secrete it into the metabolism medium. Capsule production is usually associated with virulence in pathogenic bacteria and when capsules fail to appear in Gram-positives it is noticed that the organisms when cultured grow 'rough ' and stain erratically with the Gram stain. This ' rough ' growth can be achieved in cultures by magnesium deficiency or by treatment with antibiotics. When the cells are subcultured under favourable conditions they stain strongly Gram-positive again and they are able to produce once more their surface polysaccharides. This can be shown very well with those *Streptococci* which produce dextrans and levans. It is possible that the RNA of the Gram complex is part of the enzyme system, for whilst purifying dextrans and levans and many other polysaccharides, RNA can be shown invariably to be present as an impurity which is difficult to remove by fractionation methods.

The close association of nucleic acid with polysaccharide synthesis can be shown in a much more striking manner in the *Pneumococcus* group of bacteria in which case DNA plays the vital role. DNA jackets in this group can be changed in a dramatic way and when the jackets are changed we get a change of polysaccharide overcoat also, for the DNA jackets and overcoats have to match apparently. The *Pneumococci* can be divided into different types of which about seventy have now been distinguished. The types are detected by means of their serological reactions, i.e. their reactions with immune serum. Differences in the immune sera—termed ' type specific antisera '—are due to the capsular polysaccharides which set up different reactions in the animal. Each type of *Pneumococcus* possesses a structurally different polysaccharide of high complexity and running through the whole group is one common ' somatic ' or group polysaccharide. Some of the structural differences between the specific polysaccharides are very profound, e.g. as between types 1 and 3, while others have closely similar structures, e.g. types 3 and 8. Structural similarities cause an overlap in the serological reactions and we get what are called cross-reactions as between types 3 and 8. We also get cross reactions between polysaccharides from other bacteria, e.g. the dextrans, and from other sources, e.g. plant gums, which give cross reactions with *Pneumococcal* type specific antisera, and these cross reactions can be related to similarities in structure between extraneous polysaccharides and the type specific polysaccharides.

In 1928 an Englishman, Griffiths, discovered the ' Transformation of Types ' whereby one type of *Pneumococcus* could be transformed into another thus :

Rough type 1 live cells + Heat-killed encapsulated cells of smooth type 2 ⟶ Type 2 live cells

also

Rough type 2 live cells + Heat-killed smooth type 1 ⟶ Type 1 live cells

also

Rough type 2 live cells + Heat-killed smooth type 3 ⟶ Type 3 live cells

Dawson in the U.S.A. later confirmed these discoveries and showed that any type could be transformed through its unencapsulated live form.

Alloway also in the U.S.A. dissolved the heat-killed cells in sodium cholate and showed that extracts would still achieve the transformation of types. Then came the dramatic work of Avery and his colleagues in the Rockefeller Institute showing that the factor achieving the transformation was DNA—a kind of type specific DNA in each case.

A number of types have now been transformed with the appropriate highly purified DNA. The transformed cell takes on all the properties of the new type including the property to reproduce the transforming DNA. Other bacterial types have also been transformed and also other properties such as drug resistance.

In order to understand what is happening we must consider what is known regarding the mechanism of the biosynthesis of polysaccharides. From the work of the Cori's, of Hanes and from our own laboratories, we do know a good deal about the mechanism of starch, glycogen and dextran synthesis. We know that three components are required: enzyme, substrate (a specific glucoside) and a primer. The latter must possess appropriate receptor groups and must be of the same pattern as the polymer to be synthesised and it must possess a minimum number of sugar units in its chain.

According to these requirements then the transforming DNA must possess and carry with it, its polysaccharide primer. Possibly a simple mono-layer would be sufficient and it need be no larger than an oligosaccharide. It would, however, mean as the writer conceives it that the so-called 'memory' of the transforming DNA is not solely due to its own structure but is due in part to the specific carbohydrate structure holding it rigidly in shape until it meets in the rough form of an unencapsulated *Pneumococcus* cell, a receptive protein with which it can combine to re-form the complete enzyme system for polysaccharide synthesis. It is of interest to note what remarkable differences there are between the chemical structures of the *Pneumococcus* specific polysaccharides.

One of the great fields of investigation awaiting the attention of chemists is that involving the isolation and the chemistry of nucleoproteins. Determination of the mode of linkage between these two groups of natural molecules and their isolation in a form where biological function is retained will help in solving problems connected with the chemistry of living matter —especially with chromosomes, enzymes and viruses.

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DUST AND HEALTH IN INDUSTRY

A DISCUSSION on ' Dust and Health in Industry ' was held by Section I (Physiology) of the British Association on the morning of Thursday, September 1, at Bristol. Dr. B. M. Wright of the Medical Research Council Pneumoconiosis Research Unit opened the discussion with a contribution on the measurement of dust exposure. Professor J. Gough and Dr. A. G. Heppleston of the Welsh National School of Medicine then presented a paper on the harmful effects of pneumoconiosis. Mr. S. Smith, H.M. Chemical Inspector of Factories, then discussed methods of protection against the inhalation of industrial dust. The session was concluded by Dr. R. S. F. Schilling of the Department of Occupational Health, University of Manchester, whose contribution dealt with respiratory disease among cotton workers.

An abridged version of each of these papers is printed below in the order of delivery.

THE MEASUREMENT OF DUST EXPOSURE

by

Dr. B. M. Wright

DR. WRIGHT pointed out that to understand the problem it was necessary to consider some of the physical properties of dust and some aspects of the physiological behaviour of the respiratory tract.

Airborne dust consisted of particles of such small size that their rate of settling in air was very low, of the order of 5 cm./min. for a particle 5 μ in diameter, down to rates so low as to be negligible for particles less than about 1 μ. As a consequence, the size range of airborne dust particles depended upon the atmospheric conditions ; in still air they would be very fine, whereas in a well-ventilated place relatively coarse particles would remain in suspension. Particles which were large enough to be visible were of little hygienic importance, so that it was not possible to judge the likely danger to health of an environment by visual inspection, and the use of an instrument was essential.

Turning to the physiological properties of the respiratory tract, Dr. Wright pointed out that it consisted essentially of two parts—the upper respiratory tract, which was covered with ciliated epithelium, and the respiratory bronchioles and alveoli, which were not. Particles which settled on the ciliated epithelium would be removed within a matter of minutes or hours, whereas particles which penetrated to the alveoli might be retained indefinitely in the lung. The low settling rate of very fine particles enabled them to penetrate deeply into the respiratory tract, and the finer they were the further they penetrated. The main mechanism by which they were trapped in the tract was sedimentation in the smaller bronchioles, although some loss of larger particles by impingement took place in the nasopharynx and larger bronchi. The air velocity in the respiratory tract decreased continuously as it approached the alveoli, where air movement was almost negligible, so that a substantial proportion of particles penetrating into the alveoli settled out and were retained. The proportion of particles retained was not much affected by the rate of breathing, because, although the increased air velocity tended to reduce the proportion of particles captured by sedimentation, impingement became a more important mechanism, and also the volume of air entering the alveoli was increased. It was important to realise that the penetration of particles to the alveoli had nothing directly to do with the relative dimensions of the air vessels and the particles, since the former were

never less than 200 mm. in diameter, whereas the largest particle ever likely to penetrate the alveolus was only about 5 μ in diameter. Particles which deposited in the alveoli were removed therefrom by phagocytes, and were mostly deposited in the interstitial tissue, though some probably also were carried on to the ciliated epithelium and so removed from the lungs. Particles retained in the lungs, if they were relatively insoluble, would accumulate and cause pneumoconiosis. Particles deposited on the ciliated epithelium would cause no trouble unless they were soluble and irritating. Severely poisonous substances could cause general intoxication, whereas mildly irritating materials could cause chronic conditions such as byssinosis.

Owing to the wide range of particle sizes found in airborne dust, the range to be selected for sampling must be decided by the nature of the disease under consideration. If it was thought to be mainly due to irritation of the upper respiratory tract, particles up to 10 or 15 μ diameter must be considered, whereas with a true pneumoconiosis only those less than 5 μ diameter were of importance.

Two main methods of assessing the quantity of dust were available—weighing and counting. The disadvantage of the former method was that it greatly overestimated the importance of large particles outside the respirable range, whereas the latter method was liable to be indeterminate, since the number of particles counted depended largely on the efficiency of the microscope used. In theory, a measurement of the surface area of dust might be of value, but it was practically difficult to obtain a measure of the true surface, and it was still necessary to confine the measurement to particles in the respirable size range. When collecting samples for microscopic counting, care had to be taken that they were not distorted by the sampling instrument, so that the actual size of the airborne particles or aggregates could be judged. Methods were now available for collecting samples for gravimetric analysis, in which the larger particles were separated while sampling by a mechanism similar to that in operation in the respiratory tract.

The Harmful Effects of Pneumoconiosis

by

Prof. J. Gough and Dr. A. G. Heppleston

The more severe manifestations of pneumoconiosis have, in the past, been associated with the inhalation of highly silicious dusts and in particular with their special predisposition to clinically recognisable pulmonary tuberculosis. Exposure to coal dust, on the other hand, was considered to be innocuous, since coal miners had a low phthisis death rate. Recent evidence, however, indicates that coal mining is not such a healthy occupation as was supposed and that tuberculosis plays an important part in the pneumoconiosis of coal workers.

Before discussing the means by which coal-workers' pneumoconiosis leads to disability and death, brief reference must be made to its pathological features (Gough, 1947 ; Heppleston, 1951). Two forms of the disease may be distinguished. The first or simple pneumoconiosis is characterised by numerous discrete dust foci more or less evenly scattered throughout the lung and the structural changes lead us to believe that it represents the effect of dust alone. The second form of pneumoconiosis appears as densely fibrous, black masses and is often described as massive fibrosis or complicated pneumoconiosis since we now have strong evidence that it is due to the combined effect of dust and tuberculosis. The bacteriological and histological evidence on which this view is based was derived by James (1954) who found the overall incidence of tuberculosis in massive lesions from 245 men to be 40 per cent. When split into age groups, however, 88 per cent. of massive lesions from men under forty showed tuberculosis, but only 29 per cent. of lesions from men of sixty and over. We interpret these findings as indicating the tuberculous origin of massive fibrosis and believe that with increasing age the infection dies out. Tubercle bacilli only appear in the sputum when tuberculous foci communicate with bronchi, many of which, however, are obliterated in massive lesions. This probably explains why during life tubercle bacilli are present in the sputum of only 1 per cent. of men with

massive fibrosis, as Cochrane *et al.* (1952) found. A tuberculous element therefore exists in the massive fibrosis of coal workers just as in classical silicosis, but in coal workers it is largely concealed. Why tuberculosis should be less apparent in coal workers than in silicotics is not clear, but Cummins and his collaborators (1931, 1938) showed that coal dust adsorbed tuberculin and abolished the Mantoux reaction, but quartz did not do so. Coal dust might, therefore, alter the reaction of the tissues to the toxin of the tubercle bacillus so as to restrict the extent and the progression of the disease.

Turning to the cause of death in coal workers, of 1,000 consecutive autopsy cases, 454 had massive fibrosis. Of these, 87 died from respiratory tuberculosis, but 218 from pulmonary heart disease due to vascular obliteration and emphysema associated with massive fibrosis. It can, therefore, be said that *indirectly* tuberculosis contributes greatly to the mortality of coal workers.

Although less than half of the 1,000 autopsy cases showed massive fibrosis, all of them had, in varying degree, the simple form of pneumoconiosis which must, therefore, be regarded as the primary lesion. Whilst the massive fibrosis, which characterises complicated pneumoconiosis, often leads to disability and death, simple pneumoconiosis is rarely fatal but can contribute to disability in men over the age of fifty. To establish the effect of simple pneumoconiosis on coal workers the pathological findings in the 1,000 consecutive autopsies were correlated with the degree of disability during life. Twenty-nine cases were found in which dyspnoea could be attributed only to simple pneumoconiosis, that is after eliminating the existence of all other causes for dyspnoea. This figure, however, underestimates the contribution of simple pneumoconiosis to disability because, to prove *its* effect, the 454 coal workers with massive fibrosis had to be excluded and many of these had the more severe grades of simple pneumoconiosis. To appreciate how this disability may be produced, the pathogenesis of simple pneumoconiosis must be considered.

The simple dust lesion of coal workers has three distinguishing features : a large amount of dust, a small amount of irregularly disposed fibrous tissue and frequently the presence of enlarged airspaces around the dust focus, that is, focal emphysema. Because airborne coal dust contains a little silica, 2 per cent. at the most in S. Wales (Nagelschmidt, 1943), this fraction has been regarded as the essential cause. If this were so, the pure dust lesion of coal workers would be expected to show some morphological resemblance to the classical silicotic nodule, that is the specific lesion caused by free silica. But no such resemblance exists, the silicotic nodule showing a large amount of concentrically arranged fibrous tissue with relatively little dust and no focal emphysema. We therefore regard the simple dust lesion of coal workers as a non-specific reaction to the major fraction of the dust which is devoid of free silica and whatever the free silica contributes is inconspicuous and non-specific (Heppleston, 1951). Rüttner *et al.* (1952) appear to have provided crucial evidence supporting this conclusion. Their graphite miller inhaled over a period of twenty-one years a dust with the remarkably low ash content of 0·085 per cent. The lung ash contained no quartz or compound of silicon and yet the pathological changes were exactly similar to those occurring in coal workers. It may, therefore, be said that free silica is not necessary for the development of the simple dust lesion of coal workers. In other words, the development of the simple dust lesion in men exposed to dust, such as coal or haematite, containing little free silica depends on simple mechanical accumulation of a sufficient amount of dust irrespective of its nature.

At this point the precise structure of the simple dust lesion of coal workers was demonstrated by means of a film taken from serial sections by a new technique (Heppleston, 1953, 1955).

The mechanics of focal emphysema is most easily interpreted on the basis of imbalance between the forces of inspiration and expiration (Heppleston, 1954). The simple dust lesion of coal workers is in effect a fairly wide cylinder of vesicular tissue consolidated by dust and a little fibrous tissue, disposed around the respiratory bronchioles. Dust consolidation prevents expansion of this vesicular tissue under the traction of inspiration and the

tension normally expended on these alveoli is evidently transmitted to the respiratory bronchioles as an addition to the inspiratory force to which they are normally subjected. A more important consequence of dust consolidation appears to be atrophy of the bronchiolar smooth muscle upon which expiratory contraction and shortening of respiratory bronchioles depend. These movements will therefore be diminished or abolished. Such interferences with the bronchiolar movements, both inspiratory and expiratory, will combine to produce the same effect, namely permanent dilatation, that is focal emphysema. The simple silicotic nodule, although it develops in the same situation as the focal dust lesion of coal workers, does not lead to focal emphysema because it is a progressively expanding lesion, which indents and actually narrows respiratory bronchioles.

It is now possible to suggest means by which simple pneumoconiosis in coal workers leads to dyspnoea. Consolidation of vesicular tissue by dust is probably insufficient to have much effect, but this consolidation does imply rigidity of important segments of the bronchiolar tree. Furthermore, the development of focal emphysema represents in effect an increase in the dead space of the lung, the importance of which no doubt increases as the emphysema becomes more severe. The formation of a pathological respiratory dead space is believed to be the cause of impaired haemo-respiratory exchange in non-industrial chronic vesicular emphysema (Christie, 1952).

A condition recently recognised by Caplan (1953), to which coal miners with rheumatoid arthritis are liable, is the development of so-called rheumatoid nodules in the lung. In some respects these resemble foci of complicated pneumoconiosis. The 'rheumatoid' nodules, however, show wide concentric zones and have peripheral zones of active inflammation in relation to necrotic collagen. This latter feature occurs in some nodules from which tubercle bacilli cannot be recovered by culture or guinea-pig inoculation. Rheumatoid disease is regarded by Kellgren (1952) as essentially an inflammatory reaction to necrosis of normal or pathologically produced collagen. It is, therefore, suggested that coal workers with rheumatoid arthritis react abnormally to the collagen produced in infective pneumoconiosis and the non-specific inflammatory reaction, occurring in the absence of tubercle bacilli, is believed to be the 'rheumatoid' component of these peculiar nodules (Gough *et al.*, 1955).

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Protection against the Inhalation of Industrial Dust

by

S. Smith

I intend to discuss mainly the pneumoconioses, particularly silicosis, since these diseases are so insidious and dreadful in their effect, that they deserve special attention. The principles of protection against silicosis are the same as for the obvious poisons (such as lead).

The problem is to prevent a dangerous dust from reaching the nose and mouth; and the best way is to substitute a harmless dust for a dangerous one. The classic example comes from The Potteries where alumina was substituted for flint as the bedding material in saggers and the silicosis risk has vanished from the process. Another example is the substitution of shot or other abrasives for sand in 'sand-blasting,' now enforced by law (1). Yet another is the growing use of non-siliceous artificial stones to replace sandstone grinding wheels. Even where a harmless

substitute is not available, a less harmful one might be, and so ought to be used.

But substitution is not always possible ; for example the miner is there to win coal and so substitution is not the answer to coal-miner's pneumoconiosis. The next best way is to use the dangerous material, but to enclose it so that it does not escape into the general air that people breathe. The control of radioactive dusts in the Atomic Energy Research Establishment shows how successful this method can be.

Wetting the surface is a form of enclosure and can be successful if gentle forces are at work on the material, as in the stirring of a slurry. However, if more violent forces, e.g. when a carborundum wheel cuts a block of granite, are at work, the cloud of dust is not completely suppressed by wetting. The principle of the fume cupboard which is found in all chemical laboratories may then be used. Here the apparatus is enclosed as much as possible and suction is applied to the interior of the enclosure so that air moves into the cabinet. In effect the openings are sealed, as far as dust is concerned, by a dust-proof wall of moving air. To be effective, the air, for most jobs, should move into the cabinet at about 150 linear feet per minute. Plainly, with very small openings, very little air has to be moved to give this standard of protection, so that very large machines can be protected fully for very small running costs.

Following this principle of maximum enclosure, with movement of air away from the workman towards the work, we find that, in some processes, the nearest approach to the fume cupboard that is possible has so small an enclosure that an enormous volume of air must be handled to give even moderate control of the dust. This very degenerate fume cupboard is our old acquaintance, the exhaust hood. Some recent work (2) has shown that the orthodox hood at a pedestal grinder, where a high standard of enclosure, and high speeds into the hood, are common, is all but ineffective, because much of the dust moves under the influence of the grinding wheel, rather than of the exhaust. It has been found necessary to strip this 'peripheral' dust, as far as possible, from the wheel, and to accept the fact that some of the remainder will escape from the hood, whereby the resulting external cloud can be brought under control only by applying subsidiary exhaust outside the hood. It has been found possible in some cases, such as the hollow drill (3), to incorporate the exhaust with the tool at the point where the dust is produced.

If neither substitution nor any of the forms of enclosure is practicable, or effective by itself, we are thrown on to the last resort—personal protection of each person at risk. Here we do what we ought to have done to the source of the dust : we enclose him in a dust-proof enclosure, in other words, we give him a respirator, which, if he wears it, and if it is effective, will prevent dust from reaching his lungs. The proviso about its being effective is made because some 'respirators' are not effective. Workmen put pieces of sponge into their nostrils, pieces of muslin into their mouths and cotton wool into the facepieces of the respirators ; and these 'respirators' are not necessarily effective. The easier they are to breathe through, the lower their efficiency of filtration. The normal intake of air by a workman is about 3 cubic feet per minute, and he feels some distress if a resistance of much more than $\frac{3}{4}$ inch w.g. is imposed on his breathing ; and these figures, together with details of the production of a very fine fume of methylene blue that is difficult to filter, form the basis of the British Standard for dust respirators (4). A study of this standard convinces one that the design of dust respirators is a matter for specialists.

No mention has been made of dilution of a dust cloud to bring its concentration down to a safe level. This is risky and quite wrong in principle, even though it has, apparently, legal recognition under Section 4 of the Factories Act, 1937. In a large workroom which is well ventilated the atmosphere of the whole room, however big, will become contaminated with the dust. There may be local patches near to windows and doors that are clean, but I have never found the clean spaces to be very large. Even in a lean-to shed with no walls substantial dust clouds can occur in adverse weather conditions. Moreover, it is doubtful if a diluted dust cloud is safe. Worst of all, perhaps, is the fact that dust will settle on rafters, rough patches on the walls, etc. When rafters are dusty,

not only is there the risk of local dense clouds of dust as, say, when machinery starts up and shakes the rafters, but the air in the room, in my experience, can never be free of dust. I have taken samples in a room where, for days, no process has been carried on, and still the air has been contaminated.

Adequate methods of sampling are important, and of recent years we have been fortunate in having available the thermal precipitator (5), which appears to be accurate from just below 20 μ to well down into the electron microscope range, and this ought to be the standard sampling instrument. The relatively long time over which it takes its samples is an advantage in 'smoothing out' transient irregularities in the cloud ; it makes the instrument useless for measuring clouds of short duration, such as the puffs of dust from a chisel, but alternative absolute methods, such as sedimentation, are available for measuring these clouds. Before 1936 we had no thermal precipitators, and an amazing variety of instruments came into being. One of the earliest was the Kotze konimeter (6) ; its long carefully designed jet and slow air speed make it astonishingly good, even when tested against a cloud of coal dust that contained a substantial number of aggregates. Subsequent konimeters departed from the long jet and the slow air speed with a resulting impairment of performance. Their efficiency is not only a function of particle size, but varies with different materials, and with the state of aggregation of the dust cloud.

Davies and his colleagues tested several of the common samplers (7a) and showed that impingement methods must be viewed with the utmost suspicion, although the Cascade Impactor (8) and the Bausch & Lomb Dust Counter can be reasonably efficient. But all these instruments become inefficient below 1 μ, which is an important part of the dangerous size range.

Similarly, the Midget Impinger (9) and Electrostatic Precipitator are not reliable as standards (7, 5), but will give very rough comparisons between similar dust clouds. It is saddening to think that surveys have been made, and weighty opinions given, on the strength of instruments that their users could not justify ; indeed, I suspect that many of the figures of 'maximum allowable concentrations' have been based on Midget Impinger results.

Given an efficient sampling machine, such as the thermal precipitator, we still have to take our sample ; and I am sure that it is unnecessary for me to labour the point that mere statistical sampling errors are inevitable, and might be quite high, particularly with 'snap' samplers like the ordinary konimeters, which take their sample in a fraction of a second. Having taken our sample, the number of particles of any given size that it contains has to be estimated. An error of 10 per cent. in estimating the hundreds of thousands, even millions, of particles in a sample would surprise no microscopist and there are other substantial inevitable errors. There is hope that electronic counting of the whole sample may, one day, be within the reach of all laboratories that are engaged on this work.

As I see the situation, there are five major problems in this field : I must put them in the form of questions.

(i) Are all dusts dangerous to the same extent ? Some authorities (10) and Section 47 of the Factories Act, 1937, imply that they are ; yet aluminium therapy is practised as a palliative or preventative of silicosis ; and many dusts are regarded as harmless.

(ii) Is there a critical concentration, below which damage is resisted, but above which damage is certain ? For example, might a worker in a given dusty atmosphere escape, whereas he would not escape in the same average concentration of dust if the dust reached him in dense clouds interspaced with patches of clean air, which happens to, say, a man using a chisel ?

(iii) What parameter makes a dust like silica dangerous after it has penetrated to the alveoli ? Is it surface area, freshness of surface, volume, solubility, or any combination of these ? How relevant is aggregation ? Does it merely affect the powers of penetration of the aggregate, or does it affect the intrinsic pathological potency of the dust ? The literature on this question is vast, but no certain answer is available.

And what makes dangerous dusts become less dangerous in the presence of

other materials, a phenomenon that is well demonstrated in the Kolar gold fields? (11).

(iv) What is the dangerous range size? A study of some of the best of the available information was made by Davies (12), but it was made only with the intention of discussing retention of inhaled dust in the lung. Is the dangerous size range determined completely by the lung retention curve, or is some of the retained dust relatively harmless, whereby some ought to be discounted? Modern opinion seems to tend towards the view that the graph of danger against size is not a rectangle, with no danger lying beyond some given limit, but is rather like a 'normal distribution' curve, where danger falls off on each side of a maximum. But where is this maximum? 0·1 μ and 3 μ, in certain experiments (13) have suggested themselves as very dangerous sizes, and there is a thirty-fold difference between these two figures.

Is the dangerous size range different for different people? It is already known that the health of a person's lungs affects his reaction to dusty conditions so that a tubercular man would stand little chance in some conditions that do not harm healthy people; and it is a common experience to find old men in a dangerous trade who have remained unaffected, while their healthy colleagues have succumbed to silicosis.

The efficiency of alveolar deposition is a function of the density of the dust (12); ought we then to have a different 'dangerous size range' for each material?

Now what do we mean by size? (14). Surface area plays an important part in the aerodynamics of small particles and differences between actual physical lengths lose some of their importance (15). Are we right, then, in expressing size in terms of the so-called projected area diameter (which is what we usually do when we obtain size distribution with the microscope), rather than in terms of the more appropriate parameter, the surface area diameter?

(v) The fifth problem is the problem of cure or alleviation of pneumoconiosis. Although the outlook may look bleak, the last fifty years, even the last ten years, have produced such miracles that one is, perhaps, not extravagantly foolish in hoping that an answer might, one day, be found to relieve the lot of those stricken with these dreadful diseases.

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Respiratory Disease among Cotton Workers

by

Dr. R. S. F. Schilling

Byssinosis, which occurs among workers exposed to cotton, hemp and flax dust, is a common pulmonary disease. Although first noticed among English cotton and

flax workers over a hundred years ago, the fraction of dust which causes it and its mode of action are still unknown. The disease has an odd but characteristic history. At first the worker notices tightness of the chest and breathlessness on Mondays or on the first day back at work after an absence. On the next day he has recovered completely, and recent physiological experiments have shown that among those affected there is a significant reduction in breathing capacity on Mondays compared with other working days. The symptoms may get worse and occur also on Tuesdays and then on all working days. Eventually there is severe and permanent disability.

The preparatory processes in a cotton spinning mill are dusty. Raw cotton is brought to the Lancashire mills in tightly compressed bales. The bales are opened and teased out in the cotton room. The cotton is then conveyed to the blow-room where some of the dirt and short fibres are removed. In the card-room the cotton is completely combed by the wire teeth of the carding engines and passed through a series of machines called combers (used only in fine spinning), draw, slubbing, intermediate and roving frames which draw it out into a thin strand (' roving ') ready for spinning. All men who work in the cotton and blow-rooms are exposed to dust, but the strippers and grinders who clean and sharpen the teeth of the carding engines are probably most exposed. The carder (foreman), the under-carder, the card setter and fitter, and the women ' tenters ' who mind the various machines in the card-room do not do very dusty work but are exposed to dust emitted from the carding engines. All those who work in these rooms come within the provisions of the byssinosis compensation scheme. The spinners, working in the mule- and ring-rooms, which are not very dusty, are not eligible for compensation.

Unlike the pneumoconiosis caused by mineral dusts there are no specific x-ray changes. The aetiology is obscure, since the causative agent and its mode of action are unknown. The one recorded study of the pathology by Shaw, Dunn & Sheehan (1932) does not shed any light on aetiology or help in diagnosis at necropsy, since they found chronic bronchitis and emphysema which was in no way specific in type. The diagnosis of byssinosis has to be made on the characteristic history of the sufferer. Among Lancashire cotton workers this unusual history has often been overlooked and the disease has been confused with chronic bronchitis, for which the death-rate is higher in Lancashire than in any other county in England or Wales. Considerable efforts have been made to control dust given off in the card- and blow-rooms of spinning mills. These rooms have been air conditioned, attempts have been made to extract the dust mechanically at its point of origin and oil has been sprayed on to the raw cotton to try and suppress the dust. Nevertheless, a recent investigation has shown that in mills spinning coarse cotton in the Oldham district, byssinosis is a major problem among men and women in the card- and blow-rooms. In twenty-eight mills, 115 (60 per cent.) out of 190 men aged forty to fifty-nine working in these rooms had byssinosis. 26 (14 per cent.) were assessed as being seriously disabled. Among 218 women of the same age group, 76 (35 per cent.) had the disease but only 12 (5 per cent.) were seriously disabled by it. In mills spinning the finer grades of cotton in the Bolton district, there was considerably less disease and very few of the workers were seriously affected.

Research has so far shown that the number of workers affected is consistently related to the type of work they do. There are three fractions of dust in raw cotton which may contain the causal agent of this disease—the fly, consisting of short pieces of broken fibre, fine dust derived from plant debris and the bacteria and moulds. By a combined medical and environmental survey it is proposed to relate the prevalence of disease to atmospheric concentrations of these three fractions of dust. This research should indicate the levels of dust concentrations which are safe for the cotton workers and may also help to isolate the fraction containing the agent which causes byssinosis.

Reference

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HUMAN TEETH AND ARCHAEOLOGY[1]

by

DR. E. K. TRATMAN, O.B.E.

THE selection of this title was made in order to exclude discussion of such species as the Australopithecines and the like. In the main it is intended to discuss only the teeth of various groups of *Homo Sapiens*. It may be thought that little can be gained by the study of the teeth of prehistoric man, but, in fact, their teeth can give information that will help to fill in a more detailed picture of such men, their race, their diet, their customs and habits and their diseases.

Many of the conclusions come to by such a study can only be reached by making comparisons with present-day material collected under known conditions. Due allowance must be made for the different conditions of prehistoric times. It is to be regretted that such allowance is often not made and erroneous conclusions are thereby reached.

RACE

In most surveys of the physical anthropology of tribes and groups of primitive and civilised people some reference is usually made to the jaws and teeth, but it is comparatively seldom that the teeth as entities are studied in detail. There are notable exceptions, of course. It has been my lot to have had available for study teeth of a number of different races. They have fallen into two main groups, which may be termed Mongol and Indo-European. By no means every individual will show distinctive racial characters in the teeth but in studying an appreciable number of persons some or all the racial characters of the teeth will be observed. Further, very little synthesis has been done of the many individual reports on tribal groups so it is not yet known how far it is possible to distinguish racial groups from the evidence of the teeth alone. The suggestion is made that it will be found that on the dental characters alone very few distinct groups can be made out, which, if true, would have a profound bearing on the phylogenesis of the population of the world today. It seems likely from published studies and from my own that only three groups, Mongol, Indo-European and Negro can be separated out. There are great gaps in our knowledge of the teeth of present-day man and as it is on this knowledge that our findings on prehistoric man are based the gaps in our knowledge of the latter are still greater.

It is necessary to review briefly some of the anatomical differences in the teeth of various races. A much more detailed study of the Mongol-Indo-European groups has already been published (Tratman, 1950). In this account many references are given. Other accounts will be found in individual papers but it is not necessary to review them here.

The fissure patterns of the mandibular molars are often discussed in considerable detail. They fall into two groups, the Y and + forms. The former is often dubbed the Dryopithecine form. This Y form is much commoner in Mongols than in Indo-Europeans but it is not absent from the latter nor is the + form from the former. If in a collection of teeth from a prehistoric site the Y form predominates, then it will be reasonable to conclude that the racial group had a Mongol background. A more definite conclusion cannot be drawn from this alone as tooth forms as a whole change less rapidly than the rest of the skeletal structure. The skeleton may largely have lost its Mongol characters while the teeth retain them. It must be borne in mind that a single individual may have the Y form on one side and the + form on the other. This is not a supposition but a well-observed fact.

[1] Address delivered to Section H (Anthropology and Archaeology) on September 1, 1955, at the Bristol Meeting of the British Association.

One must hesitate, therefore, to make predictions based upon a single specimen.

In the incisor teeth, particularly in the maxilla, the lateral ridges on the lingual aspect are often greatly accentuated in Mongols. Such accentuation seems to be the rule in anthropoid apes. It may be common amongst Negroes but the number of these that I have seen is too small to allow me to draw a definite conclusion. The condition is rare amongst Indo-Europeans. The form has been called both shovel-shaped and scoop-shaped. The latter is the better descriptive term. The form is not produced, as is often stated, by a simple thickening of the enamel over the ridges, but is a true morphological difference with the enamel of normal thickness. The presence of teeth of this form in a deposit would argue for a Mongol or perhaps Negro racial make up.

Neanderthal man carried this character of scoop-shaped incisor teeth. He also had another character about which much has been written, that is the molar tooth form known as taurodont. This form is usually considered as diagnostic of Neanderthal man when other evidence is missing. But only a proportion of Neanderthals do in fact show this character. The confusion thus created has been added to by the frequent failure of authors to distinguish between the true taurodont form and the reduction forms of molars in which the roots are fused together and taper towards a narrow or even pointed apex. In these, though the pulp chamber may be large and extend some way down into the root portion there is always a true root portion, which can be identified by X-ray examination or by sectioning the specimen. In the true taurodont form there is always an expansion at the root end so that the tooth seen in axial section has always an hourglass section seen in outline.

Taurodont molars occur sporadically in many racial groups and are probably not as rare as is generally thought, for it is only a small proportion of actual examples in moderns that are reported in the literature. The form occurs quite commonly in Mongol racial stock peoples, almost exclusively in the maxilla, and one should therefore hesitate to dub a tooth Neanderthal and the deposit Mousterian, or its local equivalent, merely on this single character. Other evidence should be used to date the deposit, and hence the tooth, rather than the tooth the deposit.

Another common anatomical variation seen in the teeth of Mongols are the extensions of the enamel of the crown between the roots of multiple-rooted teeth. It is rare in Indo-Europeans. The mandibular first and third molars, but rarely the second, frequently have a slender extra root situated disto-lingually in Mongols. Therefore if teeth with these variations occur in a deposit then it is fair to conclude that the people were of Mongol stock.

It is tacitly assumed in almost all cases that human teeth in a deposit represent normal individuals. But this need not necessarily be so. There must be a cause or causes for the occurrence of single teeth or of a single tooth in a deposit, but it is rarely possible to determine the cause or causes. Accident, disease or an anomaly of form are some of the possible causes. Certain types of anomaly are common amongst Mongols and it is these teeth which are often lost early through pathological processes directly associated with the anomaly and these teeth may easily be found in a deposit.

Teeth show a considerable number of racial characters, which taken in the aggregate, enable one to determine the general racial stock from which the specimens came. The evidence of the teeth may be the only evidence available as teeth will often survive in a deposit after all else has disappeared. Teeth in a deposit are often a more reliable guide to the total number of individuals represented than bones.

Though it is rather outside the scope of this paper I would like to draw attention to the popular fallacy that the presence of an ape shelf on the mandible is proof that articulate speech, as we understand it, is not possible. I have found a number of cases of well-developed ape shelves in Chinese living subjects and not one of them had the slightest difficulty over speech even in such a complicated tonal, inflection language as Chinese.

Diet and Attrition

It is generally considered that the diet of prehistoric man was harsh and tough.

This view is, in part, based on the well-known fact that all the teeth of prehistoric man, save those from very young individuals, show a marked degree of attrition both of the occlusal and interstitial surfaces. But in present-day peoples there are many whose diet is in the main soft or pappy. For example the southern Chinese and Malays depend very largely indeed on rice and vegetables. Meat and fish are added, but all are invariably well cooked and require no greater masticatory effort, if as much, as our own diet. Yet the rate of attrition of the teeth in these groups is far greater than with ourselves. It is difficult to find an adequate answer, particularly when one considers that the difference is not nearly so well marked in the deciduous dentitions as it is in the permanent.

The degree of attrition of the teeth in prehistoric man is often used as an indicator of the age of the individuals at the time of death. The assessment is usually based upon a comparison with our own teeth but, as shown above, such a basis of assessment will lead to quite erroneous results. One will see often in youngsters of sixteen the first permanent molars in Chinese, Malays and Indians worn to the stage that one hardly expects to find at the age of forty years in England. When prehistoric material is under consideration one must expect to find the enamel worn off the whole of the occlusal surface of the first molars, partly worn off, with the dentine exposed, at least under the cusps, of the second molars and appreciable wear on the third molars. The incisor teeth will usually meet edge to edge as the result of attrition and all this by the age of twenty-two to twenty-five years. In most cases the lower age will be applicable.

One sometimes encounters estimates of the age of an individual prehistoric man given as sixty to seventy or even older. In some, but not all, the estimate is stated to be based on a comparison with present-day people suffering under western civilisation. That basis of comparison is faulty particularly if, as is often the case, the degree of attrition of the teeth has been taken as a guide. There is little in the studies of primitive peoples to suggest longevity but much to suggest early maturity and early senile changes. There are of course exceptions.

Palatal measurements, including total palatal area, are often used in comparing prehistoric material with modern man and present-day primitives. The comparison with the latter is fair but with the former it often is not, for no allowance is made for the interstitial wear of the teeth in prehistoric man. This wear is very much greater in prehistoric man than in the Englishman of today. Interstitial attrition may reduce the length of the dental arch by as much as 1·0–1·5 cms. between youth and say forty years of age in prehistoric and modern primitive men. This reduction in arch length does not occur to anything like the same extent in modern Englishmen. The reduction in length of the arch must seriously affect the calculation of palatal area.

Customs and Habits

Little attention is paid to the teeth of prehistoric man from this aspect. Yet a careful study of the teeth, particularly if still set in the jaws, will yield evidence of such things as ritual mutilations by knocking out or filing down the teeth or the absence of such rituals. Evidence for the habitual use of cleaning agents on the teeth though rarely reported is probably quite common in prehistoric material. Such evidence is easily missed if the examiner is not familiar with the type of lesion produced on the teeth by varying agencies: after all, the modern tooth brush and paste, when improperly used, does produce extensive lesions. Evidence from Mendip sites clearly shows that prehistoric and Romano-British man regularly used cleaning agents on his teeth.

Tooth picks, when used habitually, will leave their own unmistakable lesions on the teeth and from these and other cleaning lesions one may be able to deduce whether the individual was right- or left-handed.

Diseases

(1) *Dental Caries*

It is well established that this disease occurred in prehistoric times, for its lesions are found in the teeth from deposits, but its incidence is low. Its pattern

is quite different from that encountered at the present time, for example, in Britain. In prehistoric material dental caries is confined almost exclusively to cavities on the interstitial surfaces of the teeth, starting at the gum margin or below. The number of cavities per person is very low. In Britain occlusal and interstitial cavities free of the gum margin are the rule and the number of cavities per person is high. Only in middle and late life do the typical lesions seen in prehistoric man generally appear. They do so partly as the result of normal senile changes and partly of pathological processes either acting alone or together. So when such cavities are observed in prehistoric material it is definitive evidence of such changes and processes having occurred in prehistoric people. If in prehistoric material a pattern of dental caries is found approaching that now found in Britain then that will be a definite indication that there had been a fairly abrupt change in the dietary regime of the people, betokening in turn a change in the living conditions.

(2) *Paradontal Disease* (Pyorrhoea)

The evidence for this disease is often clear and unmistakable in the teeth and jaws of prehistoric man. It is probable that some of the single teeth found in deposits are the result of teeth having been lost through this disease during life. There are other causes for the occurrence of single teeth in deposits, as mentioned above. A major cause is the teeth falling out of jaws while in the deposits. This is all the more likely to occur if the teeth have been loosened during life by paradontal disease. Teeth are also, alas, often allowed to fall out of jaws during the recovery of the specimens during excavations, though this is not so common nowadays. During the process of cleaning and repair such loss is easily detectable, but not after cleaning. The teeth, if recovered separately, can usually be identified at this stage, before cleaning, amongst the collection and restored to their respective jaws.

In the Romano-British people living on Mendip sites and in caves paradontal disease, with loss of teeth, was rife, and it reached a comparatively advanced stage in relatively young individuals.

Associated with paradontal disease in particular is the formation of deposits on the teeth. These will generally be of the type originally deposited below the gum margin, though the other type occurs. Quite often these deposits are mistaken for post-mortem accretions acquired while the teeth have been in the deposit and as such are removed when the specimen is cleaned up. Such deposits should not be removed, for their presence is clear evidence of poor oral hygiene and chronic inflammation of the gums during life.

(3) *Dento-alveolar abscess*

The chronic abscess leaves its mark on the jaws and teeth. Though these marks are not found very often they must have been of quite common occurrence in prehistoric people, for what is found in deposits is but a fraction of the total from the people of the period. A common cause of such an abscess is the rate of attrition, or occasionally artificial abrasion, in excess of the rate of deposition of secondary dentine within the tooth, so that the pulp becomes exposed, infected, dies and a chronic abscess is formed. Dental caries may also cause the death of the pulp and its sequelae. Another common cause is paradontal disease, with or without evidence of food impaction between the teeth. A relatively infrequent cause, surprisingly enough, is direct violence causing a fracture of a tooth and the death of the pulp. Yet one would expect such injuries to be frequent under conditions of prehistoric times.

(4) *Maxillo-Facial Deformities*

The modern science of orthodontics has grown up to deal with these. In general these deformities occur but rarely amongst modern primitives and amongst prehistoric peoples. When they do occur they are usually due to some very simple cause, such as a supernumerary tooth. On the other hand there is some evidence that protrusion of the mandible is common amongst Mongols. Again certain types such as the eruption of the anterior teeth of the mandible to a level above that of the posterior teeth do occur and examples of it can be found in the Romano-British material from some of the Mendip caves.

Summary

The teeth of man are valuable documents, and, when found in archaeological deposits, should be treated as such. It is not just a question of naming a tooth, e.g. first molar, but of making a tooth tell all that is possible about the person from whom it came. Some of the things it may be possible to tell from teeth have been described above.

Reference

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INTERNAL MIGRATION IN ENGLAND AND WALES, 1951[1]

The pattern of internal migration as revealed by an analysis of the birthplace statistics obtained at the 1951 census

by

R. H. OSBORNE

ONE of the tasks of the geographer is the study of patterns and distributions in the world around him. The location of man himself and changes in his location thus form a legitimate part and, indeed, an important part of the geographer's field of study. While only the demographer may be fully competent to handle population statistics at the more advanced mathematical levels the geographer can often contribute to their interpretation by drawing attention to the distributional and spatial aspects of population, for which, of course, his favourite tool, the map, is highly appropriate.

This paper is concerned with a cartographical presentation of some of the features of internal migration in England and Wales as revealed by the census of 1951. Ever since registration of births and deaths began in 1837 figures of internal migration have been obtainable, or at least calculable, from statistics issued by the General Register Office, the usual practice being to compare the total change in population recorded between two census years or other convenient dates with the natural increase occurring in the same period. The difference between total change and natural change gives a measure of net in-migration or net out-migration to or from the particular area concerned. Such changes by migration for periods since 1921 (1921–1931, 1931–1939 and 1939–1947) have been mapped in the 'National Atlas' series of the Ordnance Survey and have also been summarised by two authors in an article which appeared in 1953.[2] Since then the county census volumes for 1951 have been published, giving estimates of net migration for the whole twenty-year period 1931–1951.[3] The pattern of migration changes in this period (calculated by the natural increase method) has, in effect, already been dealt with by these two authors. There is, however, an alternative approach to the study of internal migration. This alternative approach provides useful confirmatory and supplementary information and is based on an analysis of birthplaces.

The 1951 county census volumes are particularly valuable in providing the only information available since 1911 regarding the counties of birth of the native population of England and Wales. From the birthplace tables (Table 19 of each county volume) it is possible (by 1,431 simple subtractions) to derive the net movements of English and Welsh persons which have occurred between pairs of counties and so, by summation, the total net loss or gain experienced by individual counties in relation to all the others combined.[4] Thus if County A contains 10,000

[1] Address delivered to Section E (Geography) on September 6, 1955, at the Bristol Meeting of the Association.

[2] E. C. WILLATTS and M. G. C. NEWSON (1953) : 'The Geographical Pattern of Population Changes in England and Wales, 1921–1951,' *Geographical Journal*, **119**, 431–450, esp. 441–446.

[3] Column (*m*) ('Balance') of Table 2 in each county volume gives a percentage change which 'represents in the main the effect of migration movements.'

[4] These are 'geographical counties,' consisting of the administrative county and any associated county borough(s). Seven geographical counties contain two or more administrative counties, the figures for which have had to be combined. The North Riding of Yorkshire, which is separated by an official regional boundary from the East and

persons born in County B and County B contains 7,000 persons born in County A the difference of 3,000 represents a net loss by migration from B to A and conversely a net gain to A from B. The sum of all the net losses and gains experienced by each county is equal to the difference between the enumerated population of the county and the total number of natives of that county living in England and Wales. Such an analysis is therefore useful in supplementing the Registrar-General's official net migration figures, especially as it offers some idea of the origins of in-migrants and the destinations of out-migrants.

Several important qualifications must be borne in mind however. For instance the birthplace method can take no account of intermediate migration occurring between departure from one's native county and enumeration elsewhere in 1951, nor can it take account of return migration. Furthermore the net balances do not relate to any definite period, although in view of the length of the average life-span it is obvious that most of the persons concerned must have migrated in fairly recent times. Thus the figures do not in themselves reveal anything about current trends : they are cumulative totals reflecting the outcome of what may well have been changing volumes of movement and changing directions of movement, with the deaths of older migrants constantly modifying the picture. Moreover, while the Registrar-General provides migration figures for the constituent local government areas of counties, the birthplace method of analysis cannot, as far as published statistics are concerned, be extended below the county level. Finally it should be remembered that neither method can treat as in-migrants the children born to newcomers after their arrival, although these are, of course, an important addition to the existing population of the receiving county.

The population of England and Wales in 1951 was about 43¾ millions, of whom rather more than 41 millions were natives. It appears that of this number some 12¾ millions (or 31 per cent.) were then living outside their native county, of whom 2¾ millions were Londoners. The counties with the largest net inward balances of migration from the rest of the country were the following :—Middlesex (850,000), Surrey (505,000), Essex (455,000), Kent (240,000), Sussex (205,000), Hampshire (155,000), Hertfordshire (145,000) and Cheshire and Warwickshire (both 140,000). Net gains of over 25,000 were also experienced by Bedfordshire, Berkshire, Buckinghamshire, Dorset, Gloucestershire, Leicestershire, Nottinghamshire and Worcestershire. The highest net losses by migration were from the County of London (1,840,000), Durham (270,000), Lancashire (245,000), Glamorgan (140,000), Staffordshire (135,000) and the East and West Ridings of Yorkshire combined (95,000). Other counties with net losses of over 25,000 were Cumberland, Derbyshire, Lincolnshire, Monmouthshire, Norfolk, Northumberland, Shropshire, and Suffolk.

These actual numerical gains and losses were, however, of varying relative importance to the counties concerned. In order to suggest the degree of impact of these net balances a percentage ratio was worked out for each county. This ' migration rate,' as it may be called, is the relationship between the net balance of migration and the total number of natives born in the county concerned, i.e. those still resident there plus those living elsewhere in England and Wales. (The total number of natives is, of course, equivalent to the enumerated English and Welsh population of the county minus its net migration balance.) Thus a migration rate of + 10 per cent. means that there has been a net intake of persons amounting to 10 per cent. of all natives, while — 10 per cent. indicates a net loss amounting to 10 per cent. of all natives.

On this basis the highest positive migration rates were recorded by Middlesex (70 per cent.), Surrey (53 per cent.),

West Ridings, has been left as a separate county, however. In addition Derbyshire, Essex and Hertfordshire are each split by a regional boundary, so that here also certain tabulations have had to be combined.

Persons born in Scotland (and Ireland also) have had to be excluded, as their counties of birth are not separately distinguished in the census volumes. (It should be noted, however, that the Registrar-General's changes by migration are not restricted solely to persons of English or Welsh birth.) Other persons have been excluded because they failed to state their county of birth in England or Wales.

Hertfordshire (35 per cent.), Essex and Sussex (both 31 per cent.), Buckinghamshire (25 per cent.), Bedfordshire (21 per cent.), and Kent (20 per cent.), while the highest negative rates were recorded by London (39 per cent.), Montgomeryshire and Radnorshire (both 27 per cent.), Pembrokeshire (17 per cent.), Cumberland and Durham (both 16 per cent.) and Monmouthshire and Rutland (both 14 per cent). (See Table I for county migration balances and rates.)

The county showing the largest number of net gains was Sussex, which gained from all the other counties in England and Wales. Second was Surrey, which lost only to Sussex, while Hertfordshire and Middlesex stood in third place with three losses each. Six other counties—Bedfordshire, Berkshire, Buckinghamshire, Dorset, Hampshire and Kent—each had between five and ten losses. By contrast Cumberland lost to all counties except Montgomeryshire and Pembrokeshire, while

TABLE I

Net Migration, 1951—*Counties*

(a) England

County	Net Migration		County	Net Migration	
	Nos.	Rate		Nos.	Rate
Bedford	+ 50,602	+ 21·3	Lincoln	− 28,881	− 4·1
Berkshire	+ 39,714	+ 11·9	London	− 1,842,374	− 38·6
Buckingham	+ 71,976	+ 25·5	Middlesex	+ 852,160	+ 69·5
Cambridge	− 2,885	− 1·2	Norfolk	− 56,661	− 9·8
Cheshire	+ 139,235	+ 13·3	Northampton	− 6,861	− 1·7
Cornwall	+ 2,097	+ 0·6	Northumberland	− 57,460	− 7·1
Cumberland	− 48,491	− 15·5	Nottingham	+ 51,980	+ 6·9
Derby	− 43,910	− 5·2	Oxford	+ 2,818	+ 1·1
Devon	+ 20,721	+ 2·8	Rutland	− 3,049	− 13·7
Dorset	+ 27,519	+ 11·2	Shropshire	− 26,470	− 8·8
Durham	− 269,826	− 16·0	Somerset	− 7,283	− 1·4
Essex	+ 453,635	+ 30·5	Stafford	− 137,434	− 8·1
Gloucester	+ 41,211	+ 4·9	Suffolk	− 46,823	− 10·0
Hampshire	+ 153,053	+ 14·7	Surrey	+ 505,007	+ 52·8
Hereford	− 11,157	− 8·4	Sussex	+ 207,495	+ 31·3
Hertford	+ 146,565	+ 35, 5	Warwick	+ 139,502	+ 8·8
Huntingdon	− 3,084	− 4·6	Westmorland	− 5,791	− 8·3
Kent	+ 240,242	+ 19·6	Wiltshire	+ 12,086	+ 3·5
Lancashire	− 242,738	− 4·8	Worcester	+ 26,967	+ 5·7
Leicester	+ 31,645	+ 5·6	York, E. and W.	− 92,986	− 2·3
			York, N.	+ 866	+ 0·2

(b) Wales

County	Net Migration		County	Net Migration	
	Nos.	Rate		Nos.	Rate
Anglesey	− 3,187	− 6·1	Flint	+ 18,660	+ 15·8
Brecknock	− 8,013	− 12·8	Glamorgan	− 138,213	− 10·6
Caernarvon	− 4,845	− 3·9	Merioneth	− 5,249	− 11·6
Cardigan	− 7,554	− 12·7	Monmouth	− 67,114	− 14·0
Carmarthen	− 18,409	− 9·9	Montgomery	− 16,494	− 26·8
Denbigh	− 7,456	− 4·3	Pembroke	− 17,802	− 17·0
			Radnor	− 7,256	− 27·3

Durham lost to all except Cumberland and Montgomeryshire. Northumberland lost to all counties in England except Cumberland, Durham and Norfolk, and Westmorland lost to all except Cumberland, Durham, Northumberland and the North Riding, although both these counties showed several gains from Welsh counties. Other English counties with only ten gains or fewer from the rest of England were Lancashire, London, Norfolk, Shropshire, Staffordshire, Suffolk, the North Riding, and the East and West Ridings combined. In Wales Montgomeryshire lost to all other English and Welsh counties except one and Radnorshire to all but five. Glamorgan and Monmouthshire made only five gains each from English counties, while Brecknockshire, Carmarthenshire, and Pembrokeshire made even fewer.

These preliminary observations demonstrate the sharp contrast between the North of England, which has been losing population, and the South of England, which has been gaining population, and more especially between the northernmost counties and those within easy access of London, although London itself has been revealed as a losing county. Wales is also prominent as an area of loss.

The first map (Fig. 1) shows for each county its overall net balance in conjunction with its migration rate. The black discs represent positive balances and the shaded ones negative balances. The migration rates have been grouped into five ranges indicated by shading of increasing intensity. Line-shading has been used for positive rates and dot-shading for negative rates, except that counties with rates falling between plus and minus 2·5 per cent. have been left unshaded.

This map of gains and losses shows a concentration of large gains associated with high migration rates in the counties surrounding London—Berkshire, Buckinghamshire, Bedfordshire, Hertfordshire, Middlesex, Essex, Kent, Sussex, Surrey and Hampshire. From the County of London there is, however, a very large net loss of nearly two millions, the negative symbol for which is partially obscured by the positive symbols for the neighbouring counties. This movement away from London, in association with the gains experienced by the surrounding counties, reflects to a large extent the normal process of inner decline and outer growth typical of all great city-regions, speeded up in this instance by the effects of war-time evacuation.[1] This outflow from London is currently being assisted by the building of the New Towns. It should be noted, however, that the total of gains in the vicinity of London more than compensates for this heavy loss, the difference being accounted for by the migration to south-east England from the rest of the country with which we are all familiar.

At this point it will be useful to examine the counties in more detail and to suggest some of the chief localities within each which have most notably contributed towards these changes, especially when large positive balances are concerned. Here a quick guide is the National Atlas series of maps mentioned earlier, which show by local government areas the Registrar-General's changes by migration in the period 1921–1947. The 1931–1951 migration changes contained in the 1951 county census volumes can also be consulted. It must be borne in mind, however, that it is quite possible for an expanding town to draw preponderantly on persons from the rest of its county rather than on migrants from other counties. Care has also been taken in the following comments not to confuse local ' overspill ' with the genuine overall growth by migration of a whole ' urban tract.'

In Berkshire a major focus of migration has been the Reading area, while in Buckinghamshire the chief centres of attraction have been Slough and, to a lesser extent, the Aylesbury and High Wycombe districts. In Bedfordshire we must cite Bedford itself but more particularly Luton, while in Hertfordshire St. Albans, Watford, Letchworth and Welwyn may be quoted from the host of places throughout this county which have shown rapid development in recent decades. Nearly all parts of Middlesex and Surrey appear to have shared in the expansion of population in these counties and it

[1] For detailed figures of net out-migration from London to neighbouring counties see a note by the author (1955) : ' Inter-county Migration in South-eastern England, 1951,' in ' This Changing World,' *Geography*, **40**, 47–48.

would be difficult to assign outstanding responsibility to particular places. There has been a growing tendency for the estuary, from Southend in the east to the Dagenham-Hornchurch-Ilford-Romford district in the west. Immediately beyond

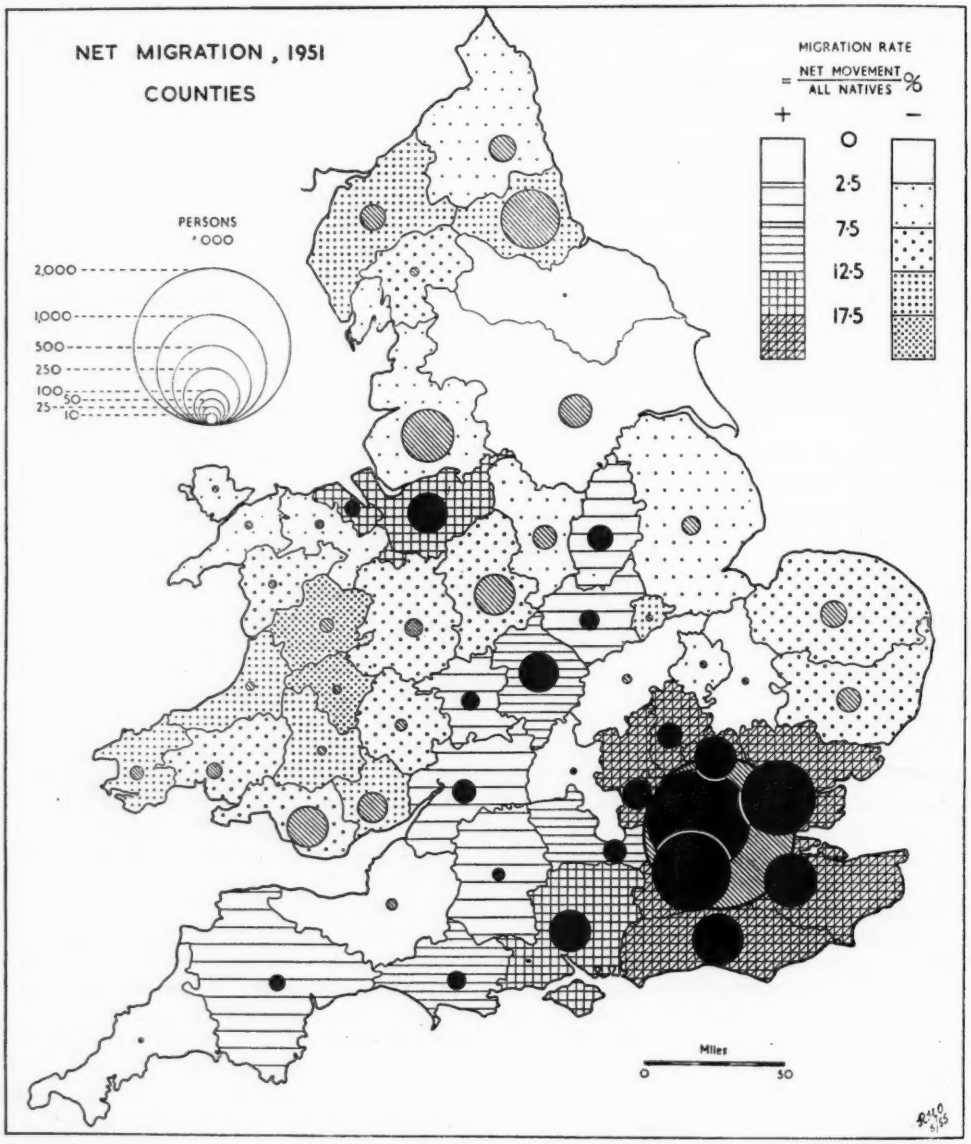

FIGURE 1

(*Note.*—Positive balances solid, negative balances shaded)

older suburbs adjoining the County of London to lose population, however. In Essex the chief area of attraction, in addition to the county town of Chelmsford, has been the north bank of the Thames the London boundary serious losses have, nevertheless, been experienced by the older suburbs, such as East Ham and West Ham.

South of the river, parts of the north

coast of Kent, including the Isles of Sheppey and Thanet, have attracted people, as have also some inland towns such as Maidstone, but the main focus of migration in Kent has been the north-west corner of the county adjoining London. Sussex, which gained from every other county in England and Wales, owes much of its high rate of expansion to the growth of its coastal towns, notably Bognor, Worthing, Brighton, Hove and Hastings. In Hampshire also the attraction of coastal districts seems to have been a chief factor leading to the large inward movement of population. Here we may mention Bournemouth and also the Isle of Wight and the Portsmouth and Southampton areas, although these two latter were adversely affected by the recent war. Persons in Defence Establishments (chiefly in and around Aldershot and Portsmouth) amounted to 5 per cent. of the enumerated population, however, and these undoubtedly form an important element in the immigrant population.[1] The development of what may be called the Bournemouth conurbation is also the chief reason for the gain experienced by the adjoining county of Dorset, which is mainly associated with the growth of Poole.

To the west of Dorset is Devon, where the sizeable net intake can be attributed to the growth of the chain of resorts stretching along the south coast from Seaton to Paignton and including Torquay. Exeter may also be included in this group. With the possible exception of Newquay, Cornwall has, on the other hand, shown no very striking growth of its coastal towns. Its overall loss is negligible, however. To the north of Devon the relatively small loss from Somerset conceals persistent migration to Bath and to the coastal towns, especially Weston-super-Mare, and also, on a smaller scale, to Taunton and Yeovil. The decline of the Somerset coalfield has been an adverse factor, and the proximity of Bristol may account for the county's net loss of 19,000 to Gloucestershire which is revealed by the birthplace tables. Gloucestershire is similar to Somerset in that notable urban growth has coincided with the decline of a small coalfield—in this instance that of the Forest of Dean. The positive migration balance results from expansion in the Greater Bristol and Cheltenham-Gloucester areas, related in both instances to the development of the aircraft industry. In adjacent Wiltshire there has been some migration in recent times to Chippenham, Salisbury and Swindon, but it is the presence of Service personnel in this county which chiefly accounts for the net inward balance (10 per cent. of the county's population being enumerated in Defence Establishments).

In the central belt of counties, stretching from the Welsh Border to the Wash, there is a somewhat confused picture of gain and loss. Both in the extreme west and the extreme east there have been losses, in varying degree, from the more rural counties of Herefordshire, Shropshire, Cambridgeshire, Huntingdonshire, Lincolnshire, Norfolk, Rutland and Suffolk. Some towns in these counties have, nevertheless, experienced inward movements of population in recent times, such as Hereford and Shrewsbury (in the west) and Cambridge, Ipswich, Skegness and especially the iron and steel town of Scunthorpe (in the east). Other towns, such as the fishing ports of Grimsby, Lowestoft and Great Yarmouth, have been net losers.

The inner counties of this central belt, which are more urban and industrial in character, show some interesting juxtapositions of gains and losses. The loss from Staffordshire can be attributed in the main to migration from the Potteries in the north and from the Black Country in the south, but it should be noted that the county's loss is more than offset by the gains achieved by Warwickshire and Worcestershire, which adjoin Staffordshire on the south. Staffordshire, in fact,

[1] The exclusion from each county population of its non-civilian element (which, it must be remembered, generally includes some persons born outside England and Wales and also some natives of the county concerned) would cause a substantial relative increase in the negative balances of Anglesey, Brecknockshire, Cambridgeshire, Herefordshire, Huntingdonshire, Lincolnshire, Merionethshire, Norfolk, Rutland, Shropshire, Somerset and Suffolk. Exclusion would cause a substantial relative decrease in the positive balances of Berkshire, Devon, Dorset, Gloucestershire and Hampshire, and would convert the positive balances of Cornwall, Oxfordshire, Wiltshire and Yorkshire (North Riding) into negative ones. For actual numbers in Defence Establishments see Table 16 of county census reports.

lost 45,000 people on balance to these two counties. Warwickshire contains the rapidly growing Birmingham-Coventry-Rugby-Leamington engineering district, while Worcestershire includes expanding suburban areas close to Birmingham, although Dudley, in the Black Country, has been losing population.

A similar effect caused by county boundaries cutting through major industrial regions is seen in Derbyshire and Nottinghamshire, where the loss from the former (masking expansion in the Derby area) and the gain by the latter can be associated to some extent with the movement of people from the East Derbyshire coalfield to the Nottingham area and the newer Nottinghamshire mines. A net movement of 23,000 over the county boundary is shown by the birthplace tables. To the south of these two counties is Leicestershire, where attention must be drawn to the growth of the central industrial zone focused on Leicester as the factor leading to the inward balance. South of Leicestershire is Northamptonshire, where despite the rapid growth of the new iron and steel centre of Corby and the progress of other towns, such as Northampton, Kettering and Peterborough, a small overall loss has been recorded. Similarly, in adjoining Oxfordshire the considerable growth of the county town in recent times is accompanied by only a very small gain for the county as a whole.

Compared with the generally favourable experience of southern England and the varied experience of central England the northern counties present a picture of almost universal loss. Large net outward movements, although not all of great relative importance, are recorded by Lancashire, the East and West Ridings, Cumberland, Durham and Northumberland. The large gain by Cheshire partially offsets the loss from adjoining Lancashire and is, indeed, yet another instance of local compensation within the same urban region, as Cheshire's gain can be linked to suburban growth south of Manchester and to the development of the Wirral peninsula as an adjunct to Liverpool. The actual net gain to Cheshire from Lancashire exceeds 100,000. The loss from Lancashire, in which virtually all its industrial cities and towns have in some degree participated, nevertheless conceals an important and uninterrupted movement of population to the coastal towns of Morecambe, Blackpool and Southport.

A general tendency towards population loss on the part of nearly all the industrial cities and towns is typical also of the East and West Ridings of Yorkshire. Here the only notable persistent gains have occurred at the resorts of Bridlington and Harrogate and at Doncaster, situated in an expanding coal-mining area. On the other hand it will be seen that the North Riding has achieved virtually complete equilibrium with the rest of the country. Here Redcar and Scarborough have shown substantial growth by inward migration. But for the presence of a large number of Service personnel (amounting to 5 per cent. of the enumerated population) the county would show a net loss, however. Cumberland, Durham and Northumberland, even more so than Lancashire and Yorkshire, were in the inter-war period characterised by economic depression. This is reflected in the particularly high outward rates for Cumberland and Durham. Among the towns of these three counties only Carlisle and Darlington and Tyneside's coastal suburb of Whitley Bay have shown continuous growth by inward migration. The non-industrial county of Westmorland also shows a net loss.

Lastly, a survey of the Welsh counties shows again the effect of extreme inter-war industrial depression. Here large losses are evident from Glamorgan and Monmouthshire, which, together with Carmarthenshire, contain the industrial and mining centres of the South Wales coalfield. Most of the other counties, suffering in some degree from remoteness and reliance on agriculture, show a considerable degree of loss, especially Montgomeryshire and Radnorshire in central Wales. The outstanding exception in Wales is Flintshire, which contains the resorts of Prestatyn and Rhyl. The relatively small degree of loss by other counties of North Wales should also be noted, since here, too, there has been a movement of population to coastal towns.

The map thus reveals the demographic

repercussions of the inter-war disparity in employment opportunities between the depressed North of England and Wales, with their highly specialised industrial and coal-mining regions, and the relatively prosperous South of England, with its newer, lighter industries and greater diversification. The map also shows some losses of population from the agricultural counties. It is apparent that conditions of fuller employment since 1939 have not yet obliterated the effects of migration in preceding years, although they must certainly have modified them. This is supported by the Registrar-General's figures of change by migration for the period 1931–1951. In some counties these conceal two opposing movements—population decline (or only negligible increase) in the period 1931–1939 and subsequent recovery in the period 1939–1951.

In addition to the general ' drive to the south ' suggested by the map there has also been a less marked and less well-known movement of population to our coasts. As has already been noted the positive balances of Devon, Dorset, Flintshire, Hampshire and Sussex can be related almost entirely to this cause, while the same factor has played a part in leading to the positive balances of Cheshire, Essex and Kent. Similar tendencies in Lancashire, Lincolnshire, Somerset and Yorkshire have been offset, however, by contrary changes elsewhere within these counties. This ' drive to the sea ' probably arises from a number of factors—economic, social and aesthetic—which cannot be discussed here, but the retirement of people to coastal towns is probably an important aspect. In some instances, of course, the reason is merely dormitory or satellite development associated with large cities (such as London and Liverpool) which are themselves situated near the coast. The result of the conbination of these two drives in their most intense form is well shown by the large gains in the block of counties stretching from Essex to Dorset. This area can perhaps be delimited, for mnemonic convenience, by a line from Colchester to Dorchester *via* Bedford and Oxford.

The currents of movement occurring between counties are often shown in map form by arrows with widths proportional to the volume of net interchange of population.[1] Such a map for the whole country on a county basis would have given a most complex and confusing impression. In the second map (Fig. 2) the counties have therefore been combined into their official regional groupings, with arrows showing the net movements which have occurred amongst the nine English regions and the Welsh region. (See Table II for migration balances and rates.)

The hatched discs represent the number of persons born in each of the regions and enumerated in England and Wales. The unshaded, detached segments correspond to net losses from the regions concerned, and the black segments, which should be considered as superimposed on their respective discs, show net gains. The Northern Region and the Welsh Region both show large net losses, represented by segments forming nearly one-eighth of their circles and corresponding, in fact, to migration rates of minus 10–11 per cent. The North-western (or Lancashire-Cheshire) Region and the East and West Ridings Region show much smaller losses, and the two Midland Regions to the south of them are in complete balance. (Their net migration figures are too small to be shown on the map.) The Eastern Region shows a loss, however. On the other hand the three regions forming the South of England all show net gains. The gain to the South-eastern Region (which includes London) is of the order of 5 per cent., the gain to the middle region—the Southern Region proper—is much larger relatively (14 per cent.), but is smaller in actual numbers, while the South-western Region has a gain which is much smaller both relatively and absolutely.

The arrows linking the regional discs are proportional in width to the net movements occurring between pairs of regions. The thinnest arrows relate to movements of between 2,500 and 7,500 persons and the largest to movements of between 142,500 and 182,500. The map

[1] See, for instance, C. T. Smith (1951) : ' The Movement of Population in England and Wales in 1851 and 1861,' *Geographical Journal*, **117**, 206, Fig. 3.

demonstrates quite clearly the southward drainage of population which was so typical of the inter-war period. The Northern Region, for instance, has lost two Midland Regions have remained in balance by drawing population from the Northern, Eastern and Welsh Regions and losing equivalent numbers to the

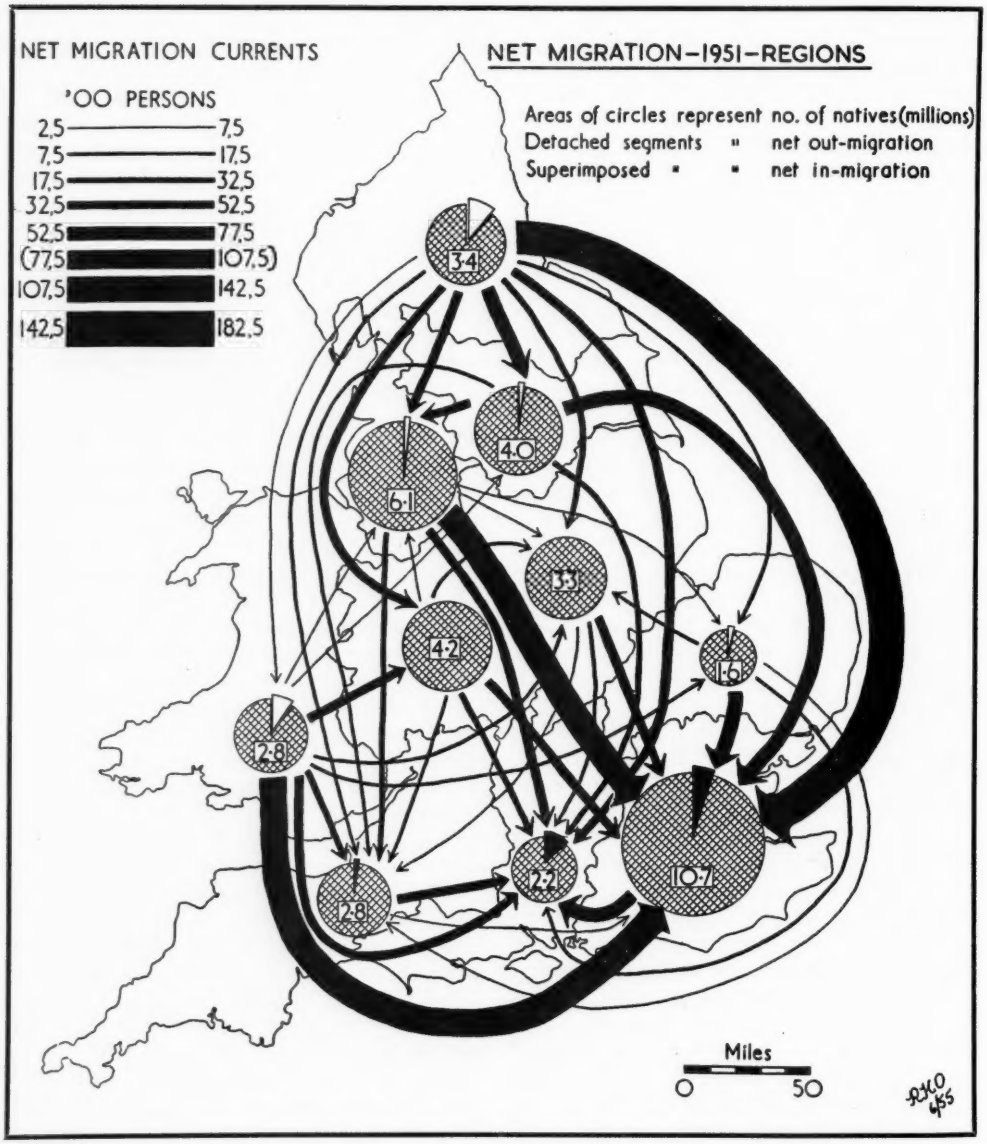

FIGURE 2

to all the other regions, but particularly to the South-eastern, while there have also been southward losses from Lancashire and Cheshire and from the East and West Ridings, with again large contingents making for the South-east. The south. Most of the migrants from eastern England and Wales have nevertheless been lost southwards. The three southern regions have thus gained from every region lying to the north of them, but the rather artificial Southern Region proper,

TABLE II

Inter-Regional Net Migration, 1951

Net gains (+) from or net losses (−) to :

Standard Region	Northern	E. & W. Ridings	North-western[1]	North Midland[2]	Midland	Eastern[3]	London & South-eastern[4]	Southern	South-western	Wales	Total	Rate
Northern	—	− 54,375	− 44,493	− 30,255	− 33,731	−11,394	− 152,217	− 34,403	− 17,312	− 2,522	− 380,702	− 11.3
E. and W. Ridings	+ 54,375	—	− 32,553	− 2,064	− 859	− 1,303	−74,414	− 25,750	− 15,716	+ 5,298	− 92,986	− 2·3
North-western	+ 44,493	+ 32,553	—	− 6,248	+ 7,437	− 6,682	− 115,630	− 38,198	− 25,307	+ 4,079	− 103,503	− 1·7
North Midland	+ 30,255	+ 2,064	+ 6,248	—	+ 11,150	+ 9,818	− 47,821	− 17,423	− 6,530	+ 13,163	+ 924	—
Midland	+ 33,731	+ 859	− 7,437	− 11,150	—	+ 1,720	− 35,883	− 20,644	− 11,852	+ 42,064	− 8,592	− 0·2
Eastern	+ 11,394	+ 1,303	+ 6,682	− 9,818	− 1,720	—	− 59,730	− 11,682	− 3,743	+ 8,463	− 58,851	− 3·7
London and South-eastern	+ 152,217	+ 74,414	+ 115,630	+ 47,821	+ 35,883	+ 59,730	—	− 68,903	+ 3,757	+ 142,181	+ 562,730	+ 5·2
Southern	+ 34,403	+ 25,750	+ 38,198	+ 17,423	+ 20,644	+ 11,682	+ 68,903	—	+ 37,135	+ 40,942	+ 295,080	+ 13·7
South-western	+ 17,312	+ 15,716	+ 25,307	+ 6,530	+ 11,852	+ 3,743	− 3,757	− 37,135	—	+ 29,264	+ 68,832	+ 2·5
Wales	+ 2,522	− 5,298	− 4,079	− 13,163	− 42,064	− 8,463	− 142,181	− 40,942	− 29,264	—	− 282,932	− 10·1

[1] Excludes North-west Derbyshire.
[2] Includes North-west Derbyshire.
[3] Excludes whole of Essex and Hertfordshire.
[4] Includes whole of Essex and Hertfordshire.

stretching from Oxfordshire to Dorset, has come out best since it has also gained from the two regions on either side—the South-western and the South-eastern. There has thus been a general southward movement of population, which has occurred not only in the form of direct migration to the three southern regions, but also in the form of what may be called subsidiary shunting movements among the other regions.

The South-eastern and Southern Regions combined, closely corresponding in area to the suggested Colchester-Dorchester zone, have gained a total of 860,000 persons from the rest of the country, while the South-western Region has gained a further 70,000. The three northerly regions, which can be considered as lying beyond a line from the Dee to the Humber, have together lost 580,000. The Eastern Region has lost 60,000 and Wales 285,000. Perhaps the best way to appreciate the significance of these inter-regional migrations, however, is to consider the hypothetical effect on the present population pattern if all migrants were returned to their native regions. The net result of such a reshuffle might then be expressed as follows. The three southern regions would have to surrender, on balance, a total population equal to the numbers at present living in Brighton, Croydon and Hendon (in the South-eastern Region), Reading and Southampton (in the Southern Region) and Gloucester (in the South-western Region). The persons thus released would provide Wales with extra population equal to that of Cardiff and Barry, the Eastern Region would gain a new Yarmouth, Yorkshire an additional Halifax, and Lancashire an additional St. Helens, while the Northern Region would gain population sufficient for another Newcastle and another Darlington.

As has already been indicated the picture revealed by the 1951 census is necessarily a reflection of experience in recent decades. Most of the migrants must have left their native county within the last thirty or forty years. Similar calculations now being made for 1901 reveal the extent to which the North of England has been dethroned as an important focus of migration during the past half-century and also the extent to which some southern counties have been rescued from a nineteenth-century condition of chronic population loss. For the last fifteen years, however, full employment has no doubt been weakening the mainsprings of migration. If full employment and planning continue over the next fifty years similar maps constructed for the year 2001 may therefore show some approximation to the kind of near-perfect equilibrium between the counties and regions of England and Wales which probably existed before the Industrial Revolution, although the continued movement of retired persons to the coast and of city-dwellers to outlying districts must necessarily qualify any such prediction.[1]

[1] In a review of the Development Plans submitted to the Government by all county authorities, in accordance with the Town and Country Planning Act of 1947, it is stated that ' the plans show that authorities do not expect any great changes in the distribution of population between northern and southern England. Within these broad areas, however, appreciable redistributions of population are expected.' (Report of Ministry of Housing and Local Government for the period 1950–51 to 1954, Cmd. 9559 (1955), p. 66.) Nevertheless the character of much of this envisaged migration is likely to be *intra*-county rather than *inter*-county.

THE SENSORY PATHWAY

SECTION I (Physiology) devoted the morning of Monday, September 5, 1955, at Bristol to a discussion of the sensory pathway. The President of the Section, Dr. R. S. Creed, Fellow of New College, Oxford, opened the symposium with his presidential address on this subject (see *Advancement of Science*, **XII**, No. 46, pp. 230–6). He was followed by Dr. G. Gordon, from the University Laboratory of Physiology in Oxford, who discussed the sensory pathway from the skin. Dr. I. C. Whitfield of the Medical School in Birmingham then presented his paper on the nervous mechanism of hearing.

The papers by Dr. Gordon and Dr. Whitfield are printed below.

THE SENSORY PATHWAY FROM THE SKIN

by

Dr. G. Gordon

SINCE there is, as far as we know, only one kind of nerve-impulse, we start to explain the fact that we have different qualities of sensory experience by making two postulates: one is that there must be functional differentiation among the peripheral sense-organs such that different kinds are predominantly sensitive to particular kinds of stimulus—a view which has considerable support; the other is that the impulses set up by these different receptors must, at some point, give rise to different central responses. In making this second point I have deliberately avoided making a common anatomical postulate—that the impulses from different types of sense-organ must be spatially segregated so that they are carried to separate specific mechanisms in the brain—because we know that one and the same collection of nerve-cells may be induced to produce different responses when differently activated by afferent impulses, and, therefore, that strict anatomical segregation is not a necessity. This is, I think, an important matter, because consideration of it would lead one to look very carefully at the physiological behaviour, and not only at the anatomical distribution, of the fibres from different sense-organs after they enter the central nervous system.

Cutaneous sensation has many qualities, and it is highly questionable whether there are reasonable grounds for distinguishing sharply between them as one distinguishes between hearing and seeing. The conventional division into touch, pain, warm and cold is quite arbitrary, and although it has proved very rewarding as a first step, it could be misleading because it tends to make one assume that if there are four senses, then there will be four kinds of sense-organ. In fact there is more variety than this in the sense-organs of the skin. To accept this convention also forces one to conclude that subjective impressions which do not precisely fit one category must necessarily be secondary and compounded of two or more primary senses—a conclusion which inevitably leads to profitless introspection and is in itself unwarranted.

These difficulties are most immediately and effectively avoided by objective study of the sensory fibres in the cutaneous nerves and of the activity of central paths. Questions of quality of sensation become, for the moment, irrelevant, and stimuli are described in physical terms. But these experiments are almost all carried out with animals: the animals, which do not survive the experiment, are anaesthetised, so that the effect of the anaesthetic becomes an important matter. General anaesthesia may have a negligible effect on peripheral nerves, but it has an important effect on synapses, so that we may expect to find, in the higher reaches of the pathway, that the picture has been simplified—not always in a way which we would have chosen—by the anaesthetic drug in question. Even after this simplification

there fortunately remains a great deal there that is interesting and relevant.

The greatest modern advances in this field of objective study have been made by Gasser, who demonstrated the different groups of fibres and their widely differing velocities of conduction in the peripheral nerves, and by Adrian, who isolated single sensory nerve-fibres and showed how they behave when the skin is stimulated. As a result, we know that small mechanical stimuli commonly give rise, in mammals, to bursts of nerve-impulses in large sensory fibres of 10–15 μ diameter—myelinated fibres conducting at 60–100 metres per second and sometimes called α and β fibres —whereas larger stimuli result in the discharge of increasing numbers of impulses in smaller fibres, particularly the δ myelinated fibres of 1–5 μ diameter conducting at velocities up to 30 metres per second, and the unmyelinated C fibres which conduct at about 1 metre per second. The impulse-discharges in the δ and C fibres are usually prolonged, which clearly shows that they arose in sense-organs in which adaptation to the stimulus was relatively slow, whereas the discharge in most large fibres shows rapid adaptation in response to a steady maintained stimulus. Frankenhaeuser (1) has shown that there are also discharges from very low-threshold receptors which adapt extremely slowly, and we believe these to travel in fibres of about 8 μ diameter (γ fibres). The point I want to make is that the physiologist can quite safely presume the existence of receptors of functionally distinct types from evidence of this kind, even if our knowledge of their discharges, especially of those from thermal receptors, is still very incomplete. I say this to excuse myself from entering into the controversial question of the structural identity of the receptors themselves.

One very important point is that a stimulus like touching with cotton-wool or rubbing with a match-stick or burning with radiant heat, or any other more or less normal stimulus to the skin, rarely seems to give rise to activity in one type of nerve-ending or nerve-fibre alone. A few δ fibre impulses are found even with light touch, although the large fibres are much more prominent; and the impressive thing, as the force of the stimulus is increased, is not that one type of fibre responds only in a limited range of stimulating energy, but rather that the *proportional size* of the contribution from the smaller fibres increases (Zotterman (2)). This gives an idea of the nature of the peripheral analysis, the central nervous system having the task, amongst other things, of analysing these proportions.

It is not surprising that a sensory message-pattern, which is not entirely comprehensible to us as we see it in the peripheral nerve, should seem less clear-cut still as we pursue it towards the brain. There is only one part of the spinal pathway of which we have any clear knowledge, and that is the compact bundle of fibres known as the *dorsal column*, which many of the larger cutaneous fibres enter without synaptic interruption, staying in it throughout their course in the spinal cord. The compactness of this column has made the study of it relatively easy. We know that it is concerned in tactile appreciation in the rabbit (Schiff (3)), and recently Phillips and I have recorded impulses from a variety of low-threshold cutaneous fibres in the upper end of the dorsal column in the cat and monkey. Gaze and I (4) were able to show that tactile impulses still reach the thalamic nucleus which relays cutaneous impulses to the cerebral cortex after the whole spinal cord had been cut through except for these columns. All this evidence agrees with the clinical view that the dorsal columns are concerned with tactile appreciation. But cutaneous impulses, both tactile and otherwise, still reach the forebrain after the dorsal columns have been cut, and the spinal paths through which these travel is obscure and certainly very complicated. We know that some of the large cutaneous fibres, and most of the smaller ones, including a very large unmyelinated *c* fibre component, find their way into the ventro-lateral column of the spinal white matter shortly after entering the cord. Most of these are on the side opposite to the side of entry, but many are on the same side. These fibres have already undergone synaptic interruption: some of them, in the primates, will then reach the thalamic nucleus in the forebrain without further interruption, but the evidence suggests that many, if not the

majority, become involved in a system made up of a number of relatively short neurones, linked synaptically, crossing and re-crossing the spinal cord, and connected at the headward end to the nuclei of the forebrain. Our experience of systems containing a number of synapses suggests that this part of the sensory pathway might behave as though very plastic, allowing impulses to pass through in some circumstances and not in others. It is certainly a very important part of the pathway: in the cat, apart from carrying impulses from low-threshold tactile sense-organs, we have found that it carries activity initiated in the δ fibres of cutaneous nerves in response to stimuli which in man might be painful. Damage to this system in man interferes with the perception of pain and temperature sensations. It is probably also concerned in mediating the spinal reflexes which follow different kinds of cutaneous stimulus and which form the motor accompaniment of certain sensory experiences: flexion reflexes, for instance, may result from painful stimuli.

It is curious that we know almost more, at this moment, about the thalamic nuclei which lie much farther headwards in the pathway. I think that one reason for this is that the cutaneous impulses, having travelled along pathways in the spinal cord and brainstem which are difficult to investigate either by anatomical or physiological methods, seem to come together again in one small nuclear region of this subcortical mass. Anatomical methods are successful here: small experimental injuries in the central areas of the cerebral cortex where cutaneous responses have been found physiologically cause cellular degeneration in this nucleus of the thalamus, so the nucleus must be sending fibres on to the cortex, and it is believed that most if not all the cutaneous impulses which are going to the cortex traverse the thalamus on their way. Physiological methods are rewarding in this region because the nucleus is small and activity concentrated, so that the electrical responses of individual cells or fibres there can be detected with a unipolar electrode with a diameter of 10 μ or so at the tip, which is guided into the nucleus by a three-dimensional mechanical drive. Another interesting feature of this region which helps the experimenter is that there is, in a general sense, an orderly topographical projection there of the surface of the body, the projection being three-dimensional, with the hindlimb in front and lateral, and the face behind and medial (Mountcastle and Henneman (5)): this projection, which was demonstrated by using tactile stimuli, calls to mind the well-known two-dimensional tactile projection of the surface of the body upon the central part of the cerebral cortex (Bard (6)).

Having taken part in some experiments on this thalamic nucleus (Gaze and Gordon (7)), I propose to spend some time discussing it: it is, in any case, interesting because here we get the last view of the cutaneous path before it enters the cortex. The question which we asked, when starting our investigation, was this: Is the thalamus just an assembly station where impulses set up in different kinds of receptors in the skin all over the body are drawn together again, after their devious paths, and wired to a kind of switchboard before transmission to the cerebral cortex? Or is it a region in which one can find evidence of sensory interaction and synthesis—phenomena which command the real interest of the physiologist? The answer, so far as we could find one, seems to lie between these two. First of all, we found that although the general conception of an ordered topographical projection of different parts of the body surface on to different thalamic regions was a correct one, there was, nevertheless, a considerable amount of overlap between, say, face, forelimb, and hindlimb in the case of the hair-tactile sense. The fact of ordered projection remains as an extremely significant feature of the system, but complete anatomical segregation is strictly a sterile conception to a physiologist, because it never allows one thing to react with another. The fact that overlap is extensive anatomically is at least compatible with functional interaction. Secondly, we found undoubted evidence that impulses set up in cutaneous fibres in a limb could cause activity not only in the thalamus of the opposite side, to which the fibres are conventionally believed to go, but also in that of the same side. In spite of some differences of opinion, this undoubtedly agrees with evidence from

other sources. This uncrossed (or ipsilateral) projection overlaps the thalamic area for the larger crossed (contralateral) projection so extensively that the two areas cannot be distinguished ; so here is another anatomical basis for interaction, this time of a more extensive kind—between skin responses from the two sides of the body.

Another thing which we set out to discover was whether there was any evidence, so much higher up in the nervous system, of the different patterns of response which, as I said earlier on, one finds in the fibres of a peripheral nerve when different kinds of stimulus are applied to the skin. We found that there was. Cells exist there which seem to respond only when hairs are moved on the surface of the skin, and as one might expect, the peripheral fibres concerned are usually large and rapidly conducting, and the central pathway also a fast one. Other cells, which are in the minority, respond when the skin is touched but not when hairs are bent, and these have usually a slightly slower pathway. Some, whose peripheral path is in the δ fibres, only respond to large cutaneous stimuli which in man might be painful. These different cells are not segregated into anatomical groups, but seem rather to be distributed at random in the region where responses from that particular part of the body surface would be expected. Here again then, among cells responding to different kinds of skin stimulus, there is an anatomical arrangement which favours interaction rather than segregation, and we have some evidence of this interaction. Some cells, though not many in our experience, respond in one way when large skin fibres are stimulated, and in another way—with shorter latency and greater consistency—when δ skin fibres are stimulated ; and this suggests that some convergent or synthetic process has already begun to take place.

When one comes to the cerebral cortex, into which many thalamic cells project, the blurring of the peripheral picture is found to have increased very greatly. There is still a very clear projection of tactile fibres, and we know from Adrian's work (8) that the impulse-patterns in the cortical fibres responding to touch and light pressure on the skin often seem very similar to those found in tactile fibres in the skin nerves. It is not surprising that the cerebral cortex should receive a large number of tactile impulses, because we commonly associate this region with the more complicated problems of spatial discrimination, and in these spatial analyses the evidence provided by tactile impulses must have the greatest importance. No activity has been found here which can be associated with cutaneous stimuli other than tactile, and it remains to be discovered to what extent the cortex is involved with sensations such as warmth, cold and pain. Electrical stimulation of the human cerebral cortex has provided no clear answer to this question, since the sensations elicited by such means bear no close relation to normal sensory experiences ; but unpleasant and even painful sensations have certainly been described by the subjects of these experiments.

In this problem of sensory qualities, the transition from what is just comprehensible to what is entirely incomprehensible seems to come between thalamus and cortex. We have no idea as yet how the patterns of nerve-impulses representing different kinds of cutaneous stimulus are presented to the cortex—or to put it another way, what sort of coding is used. Both anatomists and physiologists have so far had a common interest in pursuing segregated pathways in the nervous system, but it is probably not profitable to look, beyond a certain point, for a separate pathway or area connected with each sensory quality, especially when the number of qualities is without any clear limit. It is quite possible that different kinds of cutaneous stimulus all initially activate the same piece of cerebral cortex, but that the cells there can combine in a number of different ways : graded excitation and inhibition could produce such a state of affairs. It is of some interest that the possibility of inhibition in the pathway from the skin has not yet been seriously investigated. These unsolved problems lie in the proper field of the physiologist.

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The Nervous Mechanism of Hearing

by

Dr. I. C. Whitfield

The auditory nervous pathway commences peripherally in sensory cells which are distributed along the length of the basilar membrane. This membrane is suspended in the fluid-filled canal of the internal ear and, in fact, forms a partition across it. Vibrations of the ear drum are communicated to the fluid and hence set the membrane into vibration, thereby causing the attached sensory cells to stimulate their nerve fibres. There are some thirty thousand of these fibres distributed along the length of the membrane, some to very localised regions, others spreading a considerable distance along it. The membrane itself tapers, being much narrower at one end than at the other.

Helmholtz, who first popularised the resonance theory of hearing, regarded the membrane as behaving as a series of transverse independent strings, each with its appropriate sensory cell and nerve fibre. Any note would set into vibration one or more of these strings, much as the strings of a harp can be made to resonate in sympathy with vibrations of the appropriate frequency. The corresponding nerve fibres were activated (more or less strongly according to the loudness) and thus simply carried a picture of the already completed analysis to the brain.

An alternative theory was the so-called telephone theory, which suggested that the nerve fibres were activated in such a way that one nerve impulse was sent up the nerve for each cycle of the sound wave, and that the louder the note the more fibres were involved. Here, of course, the nerve pathway would carry a replica of the sound; analysis was presumed to be central. Both these theories have turned out to have an element of truth in them, but neither has proved to be tenable in any detail.

All studies of the mechanical behaviour of the basilar membrane, both indirect and, lately, the direct observations of Békésy, have made it evident that while high notes stimulate the narrow end of the basilar membrane rather than the wide end, and low notes conversely, there is a considerable length of membrane involved for all stimuli, and hence many hundred or even thousand fibres may be stimulated by a single tone. Thus the Helmholtz hypothesis in its pure form falls to the ground. The telephone theory received its death blow when it was found that mammalian nerves are incapable of carrying more than about a thousand impulses per second and that the auditory nerve is no exception to this. While it is true that there *is* some temporal relationship between nerve impulses and the cycles of the stimulus at low frequencies, this breaks down progressively above 1000 c/s. Numerous modifications have been suggested to meet the objections to both theories, but in neither case altogether satisfactorily.

The Helmholtz hypothesis was essentially one of spatial coding, the telephone theory one of temporal coding. While both these codes have proved untenable, there nevertheless must *be* a code; that is to say all the information about the sounds falling on the ear which eventually reaches the higher parts of the nervous system must be contained in some form at each stage of its travel up the nervous pathway. Some information may be lost on the way, but none can be obtained that was not originally present, and once lost cannot be recovered.

There is a tendency amongst neurologists to refer to the cell stations in the sensory pathway as ' relay nuclei,' as if the signals were just regenerated and handed on unchanged, as the micro-wave links hand on a television signal across the country. This idea of unchanged relaying was implicit in both the resonance and telephone hypotheses. The arrangement of the nervous pathway seems to be considerably more complex than this would warrant, as brief study will show, and, in

fact, it seems to be the case that the 'code' is changed from level to level. The analogy is rather with the convertor which changes the Continental 625 and 819 line television picture to our 405 line picture, in European exchanges. The whole picture is still there but in a different form. It is, in fact, the crux of the matter that the code must be changed from one which the ear is capable of producing, into one which the higher centres are capable of utilising.

Galambos and Davis were the first to examine the behaviour of a single channel in the auditory nerve, as opposed to that of the whole nerve trunk. They inserted micro-electrodes, with a tip diameter of about 1/10,000 inch into the auditory nerve where it enters the brain. They found, indeed, that a particular unit responded preferentially to one particular stimulus frequency, but they also found that as the stimulus intensity was raised it would respond over a wider and wider frequency band until at quite moderate intensities a response was obtained over about two octaves. This is in accordance with what might be expected from the observed distribution of activity on the basilar membrane, but not with the original Helmholtz hypothesis. Experiments recording from other parts of the auditory nerve confirm this wide spread.

The rate at which impulses pass up any particular fibre depends on the degree of excitation of the corresponding element of the basilar membrane, so that any particular note causes impulses to be discharged along a large number of fibres, one fibre carrying the highest rate of discharge and those on either side discharging progressively less rapidly as we proceed towards the edges of the excited region. The distribution is highly asymmetric, the rates falling much more rapidly towards the high frequency than the low frequency side. The number of fibres involved depends on the loudness of the stimulus but always some hundreds and often thousands of fibres are excited. We are obviously a long way from the concept of one fibre (or group of fibres) for one note.

The activity of this large number of fibres in the auditory nerve enters the first integrative cell station, the cochlear nucleus. We can form some idea of the behaviour of this nucleus by comparing its structure and its input/output relations. The nucleus is a mass of some hundred thousand or more interconnected cells, from some of which the fibres leaving the nucleus arise. Every fibre which enters the nucleus divides in a quite regular manner and connects with some hundreds of cells, and conversely each cell giving origin to an outgoing fibre has branches from several hundred inputs connected to it. Since one of these cells can only discharge an output pulse if a certain critical number of input branches connected to it are active, it therefore follows that the activity in any outgoing fibre will be a function of the activity in many incoming fibres; furthermore, the critical nature of the input pattern required will result in a fairly sharp distinction between those combinations which will produce an output in a given fibre and those which will not. The second important property of the cochlear nucleus is that of inhibition. Activity evoked in a particular fibre by a stimulus of one frequency may be inhibited if a second neighbouring tone is sounded simultaneously. The combined effect of these two properties is to delimit more precisely the active region. Small degrees of activity in the periphery are used to reinforce others, and any remaining small outputs are suppressed. Thus, instead of a wide spread of varying activity, the excitation is spread more uniformly over a band of fibres which is sharply delimited at its edges.

We thus have a block of active fibres whose position indicates the frequency, and whose number is a measure of the intensity of the stimulus. It may be asked what advantage this steep-edged distribution has over the arrangement in the auditory nerve? One answer is that the sensory nervous system in general seems to be better able to make use of a discontinuity between activity and inactivity than to locate a maximum in an active region. The second reason lies in the 'noisiness' of the nervous system. All neurones seem to have a tendency to fire off occasional impulses 'spontaneously,' that is to say in the absence of an input stimulus. If the stimulus is causing only a low rate of firing there may be confusion

caused by this spontaneous activity. This confusion is likely to become greater at higher levels in the pathway owing to the greater number of neurones involved in the chain, and so the sooner the signal is converted into a form where there is rapid discharge or none, the less likely is information to be lost in the background.

Beyond the cochlear nucleus the auditory system becomes much more difficult to study since parts of it become inextricably mixed with reflex motor and other systems outside the auditory system proper. Detailed studies have been almost entirely confined to the direct cortical pathway and that is all we shall consider here.

There can be no doubt that the auditory pathway maintains an orderly arrangement throughout, so that activity is produced at the cortex in a spatially predictable manner. By stimulating the ear with pure tones it is possible to construct a map of the auditory area with a locus for each tone. Such maps, made under anaesthesia, tend to suggest a more or less unique location for each tone, but when the cortex is unanaesthetised quite a wide area of cortex may be involved by a particular note. However the general orderliness remains.

We have seen that a particular note excites a particular block of fibres, and hence may be expected to activate a well-defined area of cortex. The movement of such an area may be followed if a number of electrodes are placed over the auditory area and the ear stimulated by a tone of rhythmically varying frequency. The movement of the active area is recorded as it passes under successive electrodes. The sound *intensity*, of course, remains constant, so that there is always activity somewhere on the cortex, but its location changes. As soon as the note becomes steady, the electrical changes due to the movement disappear, as also happens under general anaesthesia.

Very brief tones are not heard as such; the recognition of tones and the relations between tones must be regarded as something of a feat for the auditory system. As has often been pointed out, it responds much more readily to transient stimuli such as clicks and bangs. Such sounds, by their nature, excite a wide band of the system, causing a rapid and short-lived discharge in many fibres, which gets through to the cortex even under light anaesthesia. This is another example of the ready recognition of sudden changes—here a temporal rather than a spatial one.

The temporal behaviour of the auditory pathway is of interest in connection with the localisation of a sound source. If an observer wears two earphones each separately fed from two microphones situated in the ear positions of a dummy head, then a source of sound moved round the dummy appears to the observer to move equally round his own head. It has been known for many years that differences of intensity (such as would be produced by the shadowing effect of the head) can produce this effect for high-frequency notes. At low frequencies, where the wavelength is long and shadowing unimportant, alterations of phase (or time of arrival at the two ears) serve a similar purpose. What is the relation of these phase differences to the auditory response?

For low-frequency tones (up to 800–1,000 c/s) there is a definite relation between the phase of the sound and the nerve impulse in the auditory nerve even though there may not be an impulse for each cycle. This relationship is maintained certainly beyond the cochlear nucleus and probably higher. Thus far there is a basis for the telephone theory. However, the uncertainty of firing of a neurone is of the order of 0·3 msec.; that is to say the neurone may fire at any time between x and $x + 0{\cdot}3$ msec. after stimulation. It follows therefore that as the stimulus frequency is raised, the phase relations become progressively less exact until at about 3,000–4,000 c/s the relation becomes quite random. The observed response compares well, therefore, with the observations of localisation by phase which shows a similar deterioration above 800 c/s.

It is not possible in a short time even to begin to attempt to relate the great body of psycho-acoustic phenomena to the corresponding nerve patterns even were data available, which in many cases they are not, but I hope enough has been said to show how the auditory pathway acts

not as a mere cable, but as a mechanism for transforming the code represented by the mechanical response of the ear into a form which can be utilised by the 'higher centres,' and how it does this by putting the information into a code in which the individual data are represented by sharp discontinuities in time and/space. It is essentially this function of recoding into a form which can be appreciated, that I think we, in fact, mean when we talk about 'analysis' by the C.N.S., rather than the literal meaning of breaking down into component parts.

SOCIAL AND PSYCHOLOGICAL ASPECTS OF REHOUSING

SECTION J (Psychology) devoted the morning of Tuesday, September 6, 1955, at the Bristol Meeting of the Association to a consideration of certain aspects of rehousing. Dr. H. G. Maule, of the London School of Hygiene and Tropical Medicine, in a paper entitled ' The Family,' gave a description of a survey of a housing estate with special reference to changes in individual family life. Dr. F. M. Martin, also of the London School of Hygiene, then read a paper on ' The Community ' in which he dealt with the social characteristics of a new community and some of the factors affecting its development.

THE FAMILY

by

Dr. H. G. Maule

I. INTRODUCTION

THIS paper is going to discuss a small part of a very extensive study that has been undertaken by the London School of Hygiene and Tropical Medicine under the direction of Professor John H. F. Brotherston.

The overall purpose of the study was to investigate various health aspects of a community. Problems that were to be explored included such questions as : ' What is the extent of ill health ? ' ; ' What are the steps taken by the family to cope with episodes of ill health ? ' ; ' To what extent are the health services —hospitals, local authority provisions and general practitioners—meeting the demands that are made on them ? '

This paper will not attempt to describe any of these parts of the enquiry, but only draw upon a subsidiary study which is related to sociological factors involved in the well-being of a community in the process of rehousing.

II. THE AREA

In the area in which the study was carried out the local authority were building an estate of some 4,000 houses. The estate is some 15 miles from London and about 5 miles from a prosperous residential and commercial town. The site of the estate was formerly farmland, woods and golf course, and it contained a few residential country houses.

The first house was completed in December 1947, the last in September 1953. When our enquiry began there were about 150 more houses to be built and very few shops were completed.

III. THE AIM OF THIS STUDY

It is evident that when a housing estate, or a new town or any other large rehousing programme is undertaken, the people who are rehoused are affected not only in respect of the bricks and mortar which surround them. They are removed from one culture and way of life and have to adapt themselves to, or create, another. The behaviour of members of a community is determined partly by the sort of people they are, partly by the physical environment in which they live and partly by their ' social climate.' We wished to attempt to measure some of the changes that took place within the family during and following rehousing.

IV. METHOD OF INVESTIGATION

The method of investigation consisted of carrying out a series of interviews with the housewife during the first year of her life in the new community. The housing authority gave us facilities for attending their letting office when prospective tenants, having been offered a house and having viewed it, decided whether to accept it.

When the prospective tenants had actually made the decision we invited them to take part in the study and arranged to see them in their old home within the next few days. Normally a tenant was offered a house on a Friday and moved into it 8 to 10 days later. Within these 8 or 10 days we went to their old home and carried out a long interview with questionnaire.

Following the move a series of visits were made to the new home and the housewife was again interviewed. These visits took place at approximately the following intervals after the date of removal: 1 week; 1 month; 6 months, and 12 months.

V. The People Rehoused

At the time that this study took place it was the regular policy of most housing authorities to have a 'housing list.' The order of names on this list was determined by a variety of factors which might be called 'housing needs.' The sort of things that were regarded as significant factors in bringing a name to the top of the list included, for example, a family separated, health being adversely affected by present housing, overcrowding and so on.

VI. Background Attitudes

In considering the effects of changed housing it is relevant first to say something about the tenants' attitudes to the area from which they were moving. All the information given relates to eighty-eight families who were interviewed on each of the five occasions mentioned.

The things they liked about their old locality mostly related to its general convenience. Good and convenient shopping facilities (39 per cent.), 'central' position (16 per cent.), good transport facilities (15 per cent.) were all picked out as being important advantages. 'Social' attractions that were mentioned included 'nice people,' 'nice street' and 'nice houses' (8 per cent.) and easy access to places of entertainment (7 per cent.). These things are of importance when it is appreciated that transport and shopping facilities and accessibility of places of entertainment were to be least catered for in their prospective homes.

It is perhaps surprising that, living as they did in a metropolitan area, one in four of the families spoke of the advantages of the 'rural' amenities, i.e. parks, public gardens, and open spaces, which were available to them. Here at least it seemed was a kind of amenity which would not be lost to them on account of the move.

In speaking of their dislikes, a third spoke of noise, dirt or being crowded, and a further quarter spoke of 'slumminess' or the lowering social status of their district. Other dislikes in connection with which a change for the better might be expected were traffic dangers (8 per cent.) and the lack of rural amenities (10 per cent.).

When asked if they thought there was anything they would miss through leaving the district one-third said there was nothing but one in four (25 per cent.) said they would miss their friends or neighbours and the same number said they would miss the convenient shops. A few had some anxieties about moving to their new district but the great majority could pick out various aspects of their new locality that they looked forward to, e.g. 'the fresh air,' 'the garden,' 'the supposedly convenient travelling,' etc.

VII. Changes in Working Pattern following Move

One of the most apparent and most readily assessed effects of the move on the habits of the family is in terms of the wage earners' work. Some people, employed in certain public services such as bus driving and the police force, are able to continue in the same employment but to transfer to a different and more convenient geographical location. Of the 76 men who were employed at the time of their move, 7 (10 per cent.) made changes of this sort. In addition to these 7, another 20 workers (25 per cent.) changed their job following their rehousing. The remainder continued at their former place of employment throughout the year following their rehousing.

In the case of the housewives who were working at the time of their move the pattern was slightly different. Seventy-five per cent. of them (the total was 16) changed their employment.

Thus we find that rehousing seems to

have a direct effect on occupation and employment. As a rule the factor most obviously related to this change is that of travelling time and travelling cost. The *average* time travelling to and from work, i.e. daily travelling, increased by half an hour after the move. But this included 11 workers who took over 2 hours daily more than prior to their move.

In terms of expenses the average daily fares to work rose from 8*d.* before the move to 1*s.* 4*d.* after the move, an increase of 8*d.* per day. Thirty-seven men were paying more, 10 less, and for 14 there was no change, a year after this move.

In time, the average daily travelling time increased from 34 minutes to 47. As many as 16 men increased their daily travelling time by over three-quarters of an hour.

Perhaps the housing authorities have not always appreciated all of the incidental consequences that follow rehousing when the housing planning does not go hand in hand with a policy of provision of opportunity to find work.

Leaving aside the working and travelling problems of the wage earners I want to turn to some other aspects of the family's changed way of life. Two features which are particularly difficult to assess and yet are of great importance to the family are the social habits (school and play, and friendships) of the children and, for the adults, the changing links with friends and relations.

To what extent does this sort of rehousing tear up family roots and cause a weakening in all the supports that a family derives from its relations, friends and neighbours?

VIII. Children's Play and Friendships

First to consider the play habits and friendships of children. The main changes in the children's life that we could attempt to assess were in terms of their new physical environmental background and their new social relationships. Among the families studied there were 52 children between two and sixteen years old. How did their play habits alter? Perhaps it is only to be expected that the play habits of children in a busy metropolitan area will differ from children in houses surrounded by gardens and relatively quiet streets. We first enquired into the whereabouts of children during their hours of play. After the move the number of children who played outside a lot rose significantly from 60 per cent. to over 80 per cent. of the total number and this change was immediate and sustained. Nearly 80 per cent. of the children played outside *more* than they had previously, regardless of whether they had already played outside a lot before the move or not.

When mothers were asked for an explanation of the play locality of their children they gave a number of reasons; some of them (such as the fact that the child was older (37 per cent.)) being unrelated to the move. Leaving this aside, the most often given explanations were the greater safety of the garden (27 per cent.) and of the streets (40 per cent.) and the additional facilities for out-of-doors' interests derived from the close proximity of the woodland.

Comparing the playing sites before and after the move the following pattern emerges. Playing in their own or a neighbour's garden increased sixfold (15 per cent. to 86 per cent.). In the local woods or countryside changed from none to 70 per cent. of the children. The facilities of the woods as a playground, however, were not accepted by all parents as an undisguised blessing. Seven parents actually forbade their children from going there, explaining their action with phrases such as 'I don't think it's very safe' and 'we have to be careful because of recent happenings.'

But despite these facilities the number who still played in the street more than doubled (36 per cent. to 77 per cent.). There is no doubt that the street has a very great attraction as a playground for the young. When those children first arrived in their new home 'the street' had an added attraction in that building material, such as sand and gravel, was all over the place making the ideal background for exciting games. Even so, as the streets became cleared of rubble the number of children playing there did not decrease but increased, and half as many again were playing there a year after the move as were within the first few weeks.

The shortage of convenient parks was felt keenly by a number of parents, though by the end of the year nearly a quarter (27 per cent.) of the children had made some use of them. The fact that the parks were a long way off caused some parents to complain, with phrases like ' this is one of my grumbles, we need a place to keep them out of the road.'

It seemed then that the woods and open spaces, and even the gardens, did not altogether compensate for lack of handy parks, and many parents would be happier if there were a clearly defined and more controlled accessible area in which their children could play in safety.

We were also interested to explore the ease with which children were able to adapt themselves to the loss of their friends and playmates. For this purpose we enquired about children's friendships before the move, with whom did they play, and did their parents anticipate that the children would miss these friends.

Pre-move, the majority of children (60 per cent.) had special groups of friends confined to their immediate locality, and most parents (two-thirds) believed their children would miss these friends. Within 3 to 9 days of the move 60 per cent. of the children had already made one or more friends with whom they were in daily contact. Within two months 70 per cent. of the children were said to have made *special* friends, although before the move one-fifth of these children had no such special friends. In terms of their friendships with other children in the district, it is evident that the great majority of the children had settled down. The small number (11) who, a year after the move said they still missed their pre-move friends, had, even so, quickly made and retained new friends.

Parents were asked how they felt about their children's friends. Their replies were classified as positive, negative or mixed.

Parents' reactions before the move were remarkably similar to those after. Just more than half had positive attitudes in the earlier case. ' They seem nice ' is typical both before (57 per cent.) and after (59 per cent.). Just over one in ten (15 per cent. and 12 per cent.) had negative attitudes expressed by phrases such as ' they are cheeky ' and about the same numbers (12 per cent. and 14 per cent.) expressed mixed feelings such as ' good on the whole except for the language.'

These replies (together with others relating to parents' attitude to their children's friends) indicate that on the whole parents were slightly better disposed towards them in the new area than they were in the old. There was one thing, however, which was a common cause of parental anxiety about their children's friends. It was expressed by phrases such as ' the language is awful—small children are as bad as adults,' ' bad grammar and speech.' After the move, about one in four of parents made such observations compared with one in ten before they moved.

IX. Neighbours, Friends and Relatives

In attempting to assess the effects on the social behaviour of the adults, a good deal of time was devoted to asking about changing patterns of behaviour as between rehoused families and their neighbours, their friends and their relatives.

In part, the method of approach was that made by Keuper in his studies in Coventry. We were, however, less successful than he in extracting useful information from our questions about the feelings of the individual towards his neighbours. We did, however, ask a number of questions relating to actual behaviour, which we regarded as evidence of mutual neighbourly support.

The particular types of behaviour we enquired about were the following :

> Using Christian names ; shopping together ; visiting one another ; going to shows together ; giving mutual help in general or in the event of illness ; borrowing and lending ; taking care of the children.

There were, of course, a number of families who did not engage in any of the activities either before or after they moved, and another group who did one or more both before and after.

From our point of view the special interest lay in measuring the extent of change in these practices and the pattern of change that might emerge. Under every

activity enquired about the number who behaved in the 'social' way increased significantly following the move. Visiting of neighbours doubled (42 to 78); shopping together rose from 15 families to 23; shows together from 4 to 17; giving one another mutual help from 51 to 80; and help in case of illness from 55 to 64. Borrowing from neighbours increased from 46 to 62.

It might be said that much mutual help between families had its roots when each family arrived for the first time in their new homes; when they actually arrived in their new house 22 families (25 per cent.) were offered help by their 'sitting' neighbours. Within a week half of the families stated specifically that their new neighbours had been helpful. Such early assistance and mutual support was expressed in such actions as minding the children, shopping and taking messages, borrowing or lending and giving of information.

It may, therefore, be that the feature in these families' lives which had apparently produced a heightened level of social or neighbourly behaviour was the common adventure of moving into a new estate, and becoming the first occupant of a new house in it.

Yet another measure of their changed neighbour relationship is to be found in comparing the number of neighbours that a housewife could actually name. Sixteen families could give the names of fewer of their neighbours at the end of the year than they gave in their previous home, but 51 families knew more of their neighbours by name.

Thus there is a very considerable weight of evidence that the relationships that had been established with new neighbours were greatly in excess of those existing before these families made their move.

In addition to enquiring about the relationships between families and their neighbours, we also asked the housewives to distinguish between neighbours on the one hand and friends on the other. Although the distinction may sometimes be rather subtle, in general it is clear that most people regard neighbours on a purely geographical basis, the people living extremely close to them. Such people may or may not be included among their list of friends. Friends may live near or may also live at a distance and may be much more difficult to meet. Inevitably, the change in housing results in a change of neighbour. The extent to which it causes contact with friends to be disrupted is another matter. By the end of the first year the majority of our families (53) said that they now saw less of their friends. The informal and casual contacts were markedly reduced as a rule because of the time and expense of travelling. Nevertheless a majority of families (53) had more actual visits from friends, and 22 families, who had not done so before the move, put up their friends for the night. Before the move only 4 families had had friends to stay with them.

Thus, though ties of friendship were less easy to satisfy, a new pattern of behaviour in response to these ties was apparent. No doubt some of this behaviour was in response to the interest displayed by the friends in the tenants' new housing. In comparing the direction of flow of visits between friends there was a greater tendency for friends to visit the new estate than for the rehoused families to go to visit their friends. This general trend was expressed both in terms of the number of friends visiting or visited and in terms of the frequency of such visits.

A final aspect of the relationship between the rehoused families and the community may be expressed in the extent to which actual *family* ties were altered. Did the rehoused families lose contact with their relations, or were *these* ties maintained?

The pattern is very similar to that described for friends. For every one family who saw more of their relations, 5 families saw less. But even so, 70 families had more individual relations visiting them on the new estate than they had before the move, and only 4 families had fewer individual relatives visiting them. Here again, the interest displayed by their relations in their new housing resulted in a flow of visits that was outside their former experience.

As with the friends, on the new estate relations stayed for the night who had not previously done so. More than half (47) the families had relations stay for the night who had not stayed with them before the move. Before the move only

19 families had had their relatives stay with them. During the first year after the move this number had increased to 62.

X. To Sum Up

What then is the general pattern of integration with the community? First of all the social support derived from new neighbours exceeded that experienced formerly. At the same time the casual contacts with friends and relatives grew less. Nevertheless there was a flow of friends and neighbours to see the new house and to stay in it, that exceeded anything in past experience, despite the time and effort involved. To what extent these contacts will continue must remain a matter for later study.

With regard to children we find that friendships were readily made and there was little evidence that the loss of old friends was serious.

Though it seemed that the new rural amenities did not always adequately compensate for the urban equivalent, there could be no doubt that above all things parents believed that their children had gained in some way from the change in environment.

Lastly, though it has hardly been touched on, we find that there are extensive repercussions in a move of this sort on the working habits of the rehoused families. At this point I express an opinion. Planning of housing should not be divorced from industrial planning if the greatest benefit is to be derived from both.

In all the findings described in this paper the family is the central figure. In the paper to follow the focus of attention will be on the social institutions upon which these families might find some measure of dependence.

The Community

by

Dr. F. M. Martin

I. Emphases in Community Studies

It is possible to trace two fairly distinct approaches to the study of urban communities—an English and an American. Among English studies the central concern has traditionally been with the effects of adverse social and economic circumstances. Booth and Rowntree, working at the turn of the century, are widely regarded as pioneers of this type of investigation—although adumbrations, largely but by no means exclusively of a qualitative and descriptive nature, are to be found in the work of a number of brilliant administrators and journalists as far back as the eighteen-thirties. Such themes as the measurement of poverty and the definition of standards of living preoccupied English students of urban communities up to the outbreak of the Second World War.

The American approach has a briefer history but a broader scope. It has been influenced scarcely at all by economics, but very substantially by social anthropology. It must be thirty years since American anthropologists began to sigh for new worlds to conquer, and found them on their doorstep. Their influence on the study of contemporary literate communities has been largely beneficial; concerned with the study of the community as a working whole and with the interrelationships of its several parts, they have sometimes been able to achieve a rounded and penetrating understanding of a local or regional culture, a way of life.

Since the war there has been something of a fusion of these two approaches—or perhaps it would be more accurate to say that American influence on the social sciences in Britain has extended to this area of study. An important contributory factor has been the sharp decline in the incidence of poverty and unemployment, and the growing awareness that by no means all social problems were solved by the amelioration of material conditions. At any rate, British sociologists have in recent years come to display an interest in very many aspects of the structure and way of life of urban communities. The work carried out by the London School of Hygiene and Tropical Medicine is in some ways unusual, because, as is indicated in Dr. Maule's paper, our initial interest was—and indeed our primary interest still remains—in problems of health, and the choice of a community for the study of these problems was from the sociological point of view to some extent fortuitous. But once there we rapidly

developed a subsidiary interest in the housing estate as a community. We became interested both in problems of adjustment of individuals and families, consequent upon rehousing, and in the structure and development of the community as a whole.

II. Economic Standards

It was not merely a sense of loyalty to the English tradition of social investigation, however, which led us to take some account of standards of living. Perhaps the best known of all studies of rehousing —that of M'Gonigle and Kirby in Stockton-on-Tees—was directed by a Medical Officer of Health, and argued that rehousing from slum areas to a new estate produced a rapid and dramatic decline in the health of the tenants. This was said to be because the increased rents and travelling costs in the new accommodation had reduced the amount of money available to the families concerned for the purchase of food, with a consequent increase in malnutrition. M'Gonigle's statistics have been heavily criticised ; yet the work still had sufficient influence to make us feel that in spite of changes in time and circumstance the problem was not one we could ignore. Four thousand families had been rehoused on the estate with which we were concerned. The great majority of them, we knew, were paying substantially higher rentals and had appreciably greater travelling costs than before rehousing. How far had this affected their living standards in general and their nutritional standards in particular ? We gathered several pieces of evidence, all of them pointing in the same direction. The most directly relevant enquiry was a study of food purchasing and consumption habits among a small but representative sample of housewives. The report of this study gave no reason to suppose that nutritional standards had deteriorated as a result of rehousing. Confirmation was provided by a longitudinal study of physical development carried out among some four thousand school children. The schools on the estate, from whose close collaboration we have derived many benefits, have measured the heights and weights of all their pupils once a term for the past two and a half years. Analysis of the pooled results from all schools shows that the heights and weights of the estate children are, if anything, slightly superior, age for age, to those of children in the city from which they have been rehoused.

Apart from direct studies of nutrition and physical growth, we obtained a number of other indications of economic level. Serious arrears in the payment of rent, were, we found, very unusual—indeed, payment of rent in advance is a great deal more common than payment in arrears ; the survey discussed in Dr. Maule's paper showed that the average expenditure on furniture, fittings and household equipment, during the first year of tenancy was one hundred and eleven pounds per family ; while yet another study, an evaluation of housing standards, showed that the overwhelming majority of houses were at least adequately furnished, properly used and well cared for.

III. Social Composition

It was clear that, whatever the problems of this housing estate, they were not primarily problems of poverty. Here was a community which, though by no means wealthy, was at least by working class standards reasonably prosperous. The increased costs consequent on rehousing had been dealt with partly by a good deal of job-changing, so as to bring the place of work nearer home, partly by a substantial increase in the employment of married women. One immediate effect of rehousing had been to increase the average distance of the journey to work from 4 to 11 miles. But by 1954–55—when, in the course of our interview survey primarily concerned with health questions, enquiry was made about the present place of work—35 per cent. of tenants were found to be working within 4 miles of their houses, 60 per cent. within 8 miles, and 70 per cent. within 10. During the same period the proportion of married women in employment went up from about one-fifth to about two-fifths. There are of course no grounds for assuming that changes in place of work on the part of men, or entry into employment on the part of women, were brought about entirely by economic motives. But over and above these expedients it is important

to remember that this was never in any case a community living at or near subsistence level. There is a certain stereotype of municipal housing estates which dates from the period when local authority rehousing was taken to be more or less synonymous with slum clearance, a stereotype whose principal components are poverty and social disorganisation. Notions of this kind still enjoy a considerable circulation, especially in residential areas near which such estates are built, although they bear little or no relation to the realities of the present-day situation, at any rate as far as out-county estates are concerned. Neither the distribution of incomes on our estate, nor the distribution of socio-economic classes shows a preponderance of the lowest categories. Middle-class groups exist, it is true, only in very small numbers—but this is equally true of unskilled and semi-skilled workers. The numerically predominant group is the skilled worker or artisan class, corresponding to the Registrar-General's Social Class III. This category includes almost exactly two-thirds of the tenants, as compared with just under one-half of all heads of households in the County of London. Only about 6 per cent. of tenants on the estate fall into the middle-class groups above this, while in London the corresponding figure is 17 per cent. On the other hand, the semi-skilled occupations appear in almost exactly the same proportions as in London (where their incidence is in any case low as compared with the country as a whole, because of the absence of agriculture and mining) while only 9 per cent. of the tenants are unskilled workers (Social Class V), as compared with 13·5 per cent. of heads of London households. Similarly the average incomes of our tenants at the time of rehousing were a little above the contemporary national average earnings of wage-earners and were higher, too, than the average incomes of all persons applying for rehousing to the same local authority. In short, there is a good deal of selection—largely self-selection—of tenants on the out-county estates. Members of the lowest socio-economic groups—who on the face of it might be expected to have the most pressing housing problems —do not find their way there to any very great extent. The social and economic deterrents to life on an out-county estate clearly seem, in their eyes, to outweigh its advantages. When they are rehoused, it is much more likely to be within one of the inner districts of the metropolitan area. At the same time it is a plausible hypothesis that the suburb-like character of the out-county estate is most attractive to those skilled workers who are either potentially socially mobile, or at least, have acquired some middle-class or lower middle-class values and attitudes.

IV. Social Stabilisation

Economically, then, the community is tolerably well placed. In addition, there are signs that after an initially rather stormy period, it is beginning to attain some degree of social and psychological stability. For example, the rate of residential mobility—i.e. the rate at which tenants move away from the estate—increased during the early years of the estate's history, but has now become stabilised at around 5 per cent. That is to say, from an estate of four thousand families, about two hundred move away each year. If this seems fairly high, it should be remembered that the nearest town to our estate—an old-established industrial and residential centre—loses about 7 per cent. of its population by removal in an average year. Seven per cent. in fact, seems to be fairly close to the average for urban areas. The number of tenants actually leaving the estate is, of course, less than the number who would like to leave and apply to the housing authority for transfer to other areas. During the past year or two, the number of applications for transfer has dropped very steeply ; so that we may hope that once arrears have been cleared up, the actual number of removals will fall appreciably below the present rate of 5 per cent. per annum.

The same pattern of a high initial incidence rate and a subsequent decline is found in the juvenile delinquency statistics. Over the past five years the incidence of delinquency—as measured by the number of Juvenile Court appearances per thousand related population—has halved. This decline in the delinquency rate has been unbroken, from 32 per

thousand in 1949 to 28 in the following year, 25 in 1951, then 24, 18, and finally, in 1954, 14 per thousand. In each year of its history, there has been proportionately more delinquency on the estate than in the neighbouring town ; but the latter's delinquency figures are well below the national average. Juvenile delinquency on the estate is now at about the level which prevails in the city from which the rehoused families came ; in its early years, the level was a good deal higher than in the city. This suggests that it would be unwise to expect rehousing to have any very beneficial effect, at any rate in the short run, on the level of delinquency. Matters at first become a good deal worse and then return within a few years to what reasonably might be considered normal for that population. Whether it will remain constant at that level, or go on to show further improvement, must for the time being remain a matter for speculation.

We have also carried out an analysis of the records of child-guidance cases and have found, in the matter of frequency, a similarly encouraging picture of improvement. The rate at which children are being referred for psychiatric examination is now between one-half and one-third of what it was four years ago. There is, indeed, very little difference between the incidence of child-guidance cases on the estate and the incidence in the whole of the educational division of which the estate forms part.

It seems then that large-scale rehousing of this type tends to produce a short phase of social dislocation, followed by a fairly rapid recovery. Can we in addition discern any signs of a growing social integration ? After all, a housing estate of this size has the population of a small market town. How soon does such an estate develop some sort of corporate identity, and a sense of common membership in a community ? A high level of social integration could not perhaps be expected. The estate is purely a dormitory area, with no industrial development, so that the ties and loyalties developed in the work situation inevitably lie outside the community. Are there any other organisations or institutions which might help to compensate for this ?

V. The Churches

The work of the churches, though energetic, touches here as elsewhere, only a small proportion of the population. In the course of our interviews with a random sample of tenants and their wives, amounting in all to over eleven hundred persons, we found that only 10 per cent. described themselves as regular churchgoers. Even this may involve placing rather a generous interpretation on the word ' regular.' The proportions of regular churchgoers varied between denominations and between social classes. Over one-third of the Roman Catholics thought of themselves as regular churchgoers as compared with only one in twenty-five of those whose nominal affiliation was to the Church of England. The social gradient is a rather odd one and suggests that churchgoing is more frequent among the minority groups which are—by the standards of this estate—at the top and the bottom of the social scale. About 14 per cent. of the small lower middle-class group are regular churchgoers, as are the same proportion of unskilled labourers ; among the skilled manual workers who form the largest occupational class, the corresponding proportion is only 7 per cent. These social differences are partly, but not entirely, a reflection of denominational differences, Catholicism being more common among the unskilled workers and Protestant nonconformity more common in the lower middle-class group.

VI. Voluntary Organisations

The number of voluntary organisations to be found in an old-established community—societies, clubs and associations —is, of course, far greater than could be expected to arise in the brief history of a post-war housing estate, yet it is encouraging to find that a fair number of such organisations have already come into existence and have engaged the interest of a very substantial number of tenants. These include a very flourishing Horticultural Society (the very first organisation to arise), several Parent-Teacher Associations, Football, Cricket and Cycling Clubs, a Photographic Society, an Old People's Club, as well as branches of the main political parties and of the British Legion.

The functions of such associations may not always seem very far-reaching, yet they make a valuable contribution towards the social development of a community. We found that about half the tenants belonged to one local organisation or another. There were again social class differences, although the gradient in this case was a simple one. Black-coated workers were more likely to be members of organisations than skilled manual workers, and non-skilled workers very much less likely. About 60 per cent. of the tenants who in terms of occupation might be classified as middle-class or lower middle-class, belonged to one or more local organisation. The skilled manual worker class was evenly divided between members and non-members, while among the members of Social Class V, more than four-fifths belonged to no local organisation. These figures are by no means discouraging if we bear in mind the very considerable difficulties which beset such organisations on the estate—difficulties of accommodation, and difficulties in securing adequate and stable leadership.

VII. The Community Association

The Community Association—the largest and most important of the local organisations—provides an interesting example of the vicissitudes of a voluntary association. It came into existence in the early days of the estate, primarily as a pressure group, concerned with agitating for various amenities. It should perhaps be explained that the essential feature of these estates is that they are built and the houses are owned by local authority A, but within the area of local authority B. The rehousing authority does not regard itself as having responsibilities to its tenants which are very much greater than those of a private enterprise developer of housing. This may seem a hard view, but it is to a considerable extent a defensible one for, given the normal divisions of responsibility in local government, a continuing social function could not be exercised by the rehousing authority except within very narrow limits. There is of course no special body corresponding to the Development Corporation of a New Town, and the responsibility for the provision of all local authority services falls on the receiving authority. The receiving authority in this case has done its job well. It has built excellent new schools and is now building a health centre ; in all, it has undertaken capital expenditure on the estate to the extent of well over a million pounds. But, because of the different authorities involved, there has almost inevitably been a time lag between the building of houses and the provision of other services. It was in the very difficult early days that the Community Association began its agitation. Its motives were widely regarded as being political. This, I think, is partly true, but to suppose that this is the whole explanation would be to under-estimate both the genuinely unsatisfactory feature of the situation and the tendency of such situations to bring to the fore certain rather aggressive types of personality who do not necessarily have political affiliations at all. In due course, however, all the services and all the amenities were provided. Perhaps one or two of them came along as a result of the Community Association's pressure just a little earlier than they might otherwise have done. During this period the Association had undergone a series of internal crises and changes of leadership and had lost many friends. It would not have been surprising if, in the circumstances, with its original impetus gone, it had quietly crumbled away. Instead, it is pleasant to report that it has found a new strength. Instead of remaining an association of individuals, it has set out to secure the affiliation of as many as possible of the other voluntary bodies on the estate. In this it has achieved a fair measure of success, and shows promise of becoming a virile and democratic body, representative of the whole estate.

VIII. To Sum Up

Our enquiries suggest that the increased costs of life on an out-county estate are fairly well tolerated, at any rate in this particular community. Many men take up work nearer home, and quite a lot of married women find jobs for themselves. But we note that in any case income levels in our families tend to be above the average ; certainly their initial expenditure

on furniture and house furnishings does not suggest that serious economic difficulties are very widespread.

The income level of the estate population is probably associated with its peculiar social composition. Although the middle-class is, by London standards, grossly under-represented, so too is the unskilled labouring class. Two-thirds of all tenants are skilled workers.

Measurements of the extent of residential mobility, of the incidence of juvenile delinquency and of the incidence of child guidance cases, all suggest that after an initial period of social dislocation, a fair degree of stability has been attained. Whether good housing will in due course bring the incidence of delinquency below the London level still remains to be seen.

The impact of the churches on the estate—if seen in numerical terms—has been limited. Only one adult in ten is prepared to describe himself as a regular churchgoer. The denominations vary a good deal in effectiveness of their appeal and different social classes on the estate vary a little in their responsiveness.

Half the adults on the estate belong to one or more of the clubs and societies which function there. A marked social gradient in the extent of participation is perceptible here as in other studies. A Community Association has been active since the early days of the estate. It began as a pressure group, and has suffered many changes of leadership ; it is now trying very hard to transform itself into a representative organisation.

It should be emphasised that a great deal of examination and analysis of material remains to be carried out, and that the foregoing pages cannot be regarded as more than an interim report, selective rather than comprehensive.

SECTION E.—GEOGRAPHY

GEOGRAPHICAL GLOSSARY COMMITTEE

REPORT for 1954–55 of COMMITTEE appointed to prepare a glossary of geographical terms with agreed definitions in English, including reference to origin and to current use and misuse.—Prof. L. DUDLEY STAMP, C.B.E. (*Chairman*), Mr. G. R. CRONE (*Secretary*), Prof. H. C. DARBY, Prof. W. G. EAST, Mr. E. O. GIFFARD, M.B.E., Mr. A. F. MARTIN, Prof. A. A. MILLER, Dr. C. J. ROBERTSON, Mr. C. T. SMITH, Dr. G. T. WARWICK, Prof. S. W. WOOLDRIDGE, C.B.E., Mr. R. A. SKELTON.

DURING the year the Committee has carried out the main part of its work by correspondence and has been working in accordance with the programme previously agreed and detailed in the Report for 1953–54. Through the generosity of the London School of Economics and a private donor, the Research Officer, Mr. W. T. W. Morgan, M.Sc. (Econ.), who began work last session, has worked throughout the whole of this year till his appointment terminated on July 31, 1955.

A list of terms, from A to Z, was circulated to members of the Committee and Mr. Morgan, taking into consideration their recommendations for additions and deletions, has dealt fully with approximately 800 words.

When this work was almost complete, the Chairman called a meeting of the Committee at the rooms of the Royal Geographical Society on June 15, 1955. Owing to the railway strike, the attendance was small but full minutes were afterwards circulated to members.

At this meeting the Chairman said Mr. Morgan had collected much valuable material in the extracts illustrating usages and that he had combed a wide variety of sources. The Chairman said he was in favour of the publication of a comprehensive geographical glossary by a commercial firm, subject to discussion with the British Association. The Committee concurred with this opinion, and proceeded to discuss the scope of the glossary. It was considered that it should contain terms which geographers might expect to find, but that it should not cover comprehensively the terms of related subjects, e.g., cartography or meteorology. Foreign terms which had become 'naturalised' or used in current literature (e.g., dolina, mesa) in the original form might be included, but if they were normally translated into English equivalents, the original foreign words should not be included.

The preface should contain a full statement of the scope and aim of the work and a history of the Committee and its labours, and there would be a full bibliography of sources. The arrangement of the text should follow the form agreed (see Report 1953–54), except that the Committee would not attempt to reach agreed definitions, but, at the end of each entry, would comment on the usage as revealed by the quotations and make appropriate suggestions.

The following procedure for the future was approved: A final list of words would be circulated to members of the Committee asking them:

(i) to add any terms they wished to be included, but not to delete terms at this stage, and making it clear that this list would be taken as the Committee's final decision.

(ii) to mark those particular terms in which they are interested, and the entries which they would like to read. These would be sent in due course.

(iii) to return the entries with any comments they may wish to make.

The inclusion or exclusion of doubtful terms was left to the discretion of the Chairman.

The Secretary, in consultation with the Chairman and Mr. Morgan, undertook to draw up an appropriate title-page and preface for the glossary, which would be circulated to the Committee.

The Committee wish to express their appreciation of the excellent work done by Mr. W. T. W. Morgan as Research Officer.

Mr. Morgan worked on the lines set out above until July 31, and the final list of terms will shortly be circulated.

L. DUDLEY STAMP,
Chairman.

G. R. CRONE,
Secretary.

THE INFLUENCE OF PESTICIDES ON THE BALANCE OF ARTHROPOD POPULATION[1]

by

DR. W. E. RIPPER

IN the majority of cases when pesticides are sprayed on crops to control phytophagous insect outbreaks, the population of the pest is reduced without being followed by alarming repercussions on the part of the insect and mite population.

But the natural environment is an unstable system. The number of species of insects and mites present varies and the interrelationship amongst the constituent species is complex. Since the interdependence of the various insect species leads to an equilibrium—a ' balance '—a removal of one or, more frequently, of several species by the application of pesticides can have profound influence on the other insects and mites present.

Such disturbances of the balance of insect and mite population can be put into two groups : ' short-term ' effects which take place during one vegetation period, of which the most notable is the resurgence of pest population soon after the chemical treatment ; and the more insidious ' long-term ' effects which become obvious only after a number of years, sometimes culminating in the appearance of strains of pests resistant to the pesticide.

These repercussions constitute a major weakness in the chemical control of pests which has raised the question whether it is not an overrated method which is indeed rapidly stultifying itself.

As chemical control is for all but a minority of pest outbreaks the only immediately practical control method so far available, this weakness, of course, must be urgently remedied. Ecological studies, research into alterations of the application method in accordance with results of these studies, and modification of the toxicological approach offer us real promise that this weakness will be overcome in time. I hope to give you in this paper a general picture of these problems of chemical control, but at the outset I would urge you to keep them in the right perspective by never forgetting the many successful and uneventful chemical control operations which are being carried out every year on a vast acreage of crops, producing an enormous increase in the world's food supply.

[1] Address delivered to Section D (Zoology) of the British Association at Bristol on September 7, 1955.

I. FIRST SEQUEL

First, let me take the resurgences of pest population after a crop has been sprayed. A great number of insecticides, fungicides and even nutrients and inert diluents when applied to orchard or field crops at times bring about a tremendous increase of the pest against which they have been directed or an equally troublesome increase in the population of other phytophagous species which were present at the time of treatment only in unimportant numbers. Resurgences have been recorded for over fifty species of phytophagous insects and mites belonging to the families of red spiders, gall mites, Tarsonemid mites, scale insects, Aphids, whiteflies, Noctuid moth, Tortricid moth, fruit flies and springtails; such resurgences occurred in temperate, sub-tropical and tropical climates.

There are in the world about 5,000 insect and mite species of economic importance—that is, which cause serious losses to farmers and growers. Of these about fifty species have so far shown resurgences, that is, roughly 1 per cent. This might not sound much, but unfortunately a good number of them are ubiquitous and very injurious.

The wide spread of this phenomenon of resurgence among different groups of

phytophagous insects and mites and its occurrence after the application of several pesticides with different modes of action and in a wide variety of climates, indicates that chemical control in these cases upsets the population dynamics. To explain what happens, three hypotheses have been put forward :

(1) The reduction of natural enemies, because the pesticide kills them at the same time as the pest.
(2) Favourable influences of pesticides on phytophagous insects and mites.
(3) The removal of competition.

(1) Many species of predatory—that is, flesh-eating—insects and mites feed on the plant-feeding insects and mites. In addition, some flies and minute wasp-like hymenoptera lay their eggs on or in other insects—many of which are pests—and their parasitic larvae develop on or in their hosts and kill them. Some of these predators and parasites are in the habit of staking their prey when comparatively few individuals of the pest species are about and thereby do a good job in preventing pest outbreaks. They are called ' low density dependent natural enemies.' Others are attracted only if the infestation of pests is heavy, and help in suppressing outbreaks which have occurred. They are called ' high density dependent natural enemies.' These natural enemies often form an effective brake on the development of large populations of injurious insects. In the ecologist's terminology, the biotic resistance of such ecosystems is high.

(2) Unfortunately many insecticides and fungicides now in general use have a destructive effect on the natural enemies of the phytophagous species and often eliminate the predators and parasites.

The criteria required to justify the conclusion that the removal of parasites and predators is responsible for the resurgence of the pest population, are : (*a*) a negative correlation between the number of natural enemies and pests ; (*b*) proof that the predators were the limiting factor to the density of the pest population ; and (*c*) an exclusion of favourable influences of the pesticide on the pest.

(3) There are many records of negative correlation between the population density of pests and their natural enemies in resurgences of pests (DeBach, Bartlett, Collyer, Huffaker, Massee and Wallace).[1] I give now one instance only—the effect of pesticides on the balance of the strawberry mite population (*Tarsonemus pallidus*), and its predator, the mite Typhlodromus.

In order to discover the role which the natural enemies play as a brake on the development of the strawberry mite population, Huffaker and Kennett removed this predator in two entirely different ways, by hand and, chemically, by parathion. In both cases, artificial predator removal caused the mite population in the predator-free group to increase by a similar factor, that is fifteen to thirty-five times, thus providing evidence that the increase was in both cases a direct result of the removal of most of the predators, so that the possibility that the chemical itself stimulated the strawberry mite can be excluded. In this case, we are therefore justified in saying that the resurgence of the pest was caused by the elimination of the natural enemies by parathion.

II. Second Sequel

Resurgences of phytophagous species other than the pest to be controlled.

Spray applications are sometimes followed by serious outbreaks not of the pest against which they were used but of other insects and mites, which prior to the treatment were so rare as not to be economically important ; such resurgences are sometimes criticized as entomological blunders and described as ' man-made ' pest outbreaks.

An examination of these resurgences revealed in many cases again a negative correlation between the population density of the resurging plant-feeding species and their natural enemies.

A good example was furnished by Massee, who has made a life-long study of British fruit pests and the effect of sprays on insect populations of orchards. He reports that the fruit-tree red spider mite (*Paratetranychus pilosus*) occurred in small

[1] A complete list of the more recent literature on this subject may be found in a chapter under the same title by the author in the Annual Review of Entomology. Vol. 1, 1956, from which this synopsis has been prepared.

numbers in many commercial apple orchards in the south-east of England, but until 1922 not in economically significant numbers. Its population was first seen to increase in commercial orchards in 1923 after winter sprays with tar oil had been introduced. Tar washes are not toxic to the wintering eggs of the mite, but they killed the predators of the mite including the Anthocorid bugs, the ladybird beetles and predacious mites. This destruction largely accounted for the rapid build-up of mite population in the spring after their emergence from the wintering eggs.

Time does not allow me to give you other recorded examples of a negative correlation between the population density of a ' man-made ' pest outbreak and their natural enemies after insecticide applications, but it is important to keep in mind that this undesirable effect on the natural enemies of phytophagous insects and mites is also produced by fungicides. Sulphur, for instance, has a detrimental effect on the predacious Typhlodromid mites which so often keep down red spider mites (Cutright, Flanders, Garman & Townsend, Lord, Clancy & Pollard).

Of the organic fungicides, the carbamates have a detrimental effect on the predacious Typhlodromid mites. Heavy red spider infestation follows treatment with carbamates, and trees sprayed with carbamate showed fourteen times the red spider infestation of the neutral copper fungicide sprayed or unsprayed trees (Lord and Mathys).

To prevent mass outbreaks of ' man-made ' pests, it has become customary to combine a further pesticide in order to prevent the resurgence of the ' man-made ' pest. In the past such combination of pesticides was selected without regard to their influence on the natural enemies of the phytophagous insects and mites present; so that while they combat the resurgence of ' man-made ' pests, they also decrease their natural enemies in the habitat and hence are likely to accelerate the appearance of long-term effects on the balance of insect and mite population which we shall presently discuss. Therefore in any ecosystem in which natural enemies of plant-feeding species are present, such combination of pesticides must be regarded as an interim remedy only

What are the long-term effects of pesticides on the balance of insect and mite populations ?

The cumulative effect of non-selective pesticides applied over a number of years, is obviously a fauna poorer in predators and parasites of low host density dependent species, while the high density dependent species re-establish themselves quickly and follow the resurgences, often in large but strongly fluctuating numbers. In such an ecosystem with its reduced biotic resistance and symptoms of unbalance, resurgences of pest outbreaks after spray treatment become chronic. They occur regularly, even under less favourable climatic conditions, and they are distinguished by outbreaks of ' man-made ' pests. These sequels have in the past often induced the grower or farmer to increase the frequency of pesticide applications in the hope of maintaining satisfactory control.

Some research workers have studied the long-term effect of pesticides which have been used regularly over an extended period of time. Monthly spraying of wettable DDT on citrus groves in California caused a severe disturbance to the balance between the California red scale (*Aonidiella aurantii* Mask.) and its predator (*Aphytis chrysomphali* Mercet) (DeBach & Bartlett). The dosage rate of DDT was insufficient to inhibit the increase of the scale population, but it was extremely toxic to the parasites and predators; the result was that parasite activity was suppressed for eleven months and in the course of this period the average red scale population density increased in one of the two groves so treated by one hundred and forty times and by thirty-six times in the other. To check whether reduced parasite activity was the cause of this, the parasite activity was suppressed on further plots by other means, namely by giving free access to Argentine ants who guard the scale and fight the parasites off their charges; this led to a red scale population density twenty times higher than on trees where the ants had been controlled by eighteen prior monthly applications of chlordane to the soil.

DeBach & Bartlett made observations of outbreaks of cottony-cushion scale over a two-year period subsequent to DDT

application, and noted that in groves where DDT treatment against citrus thrips and citricola scale had been applied previously but the spray residue had weathered sufficiently, re-entrance of the predacious vedalia beetle occurred; this predator soon killed off excess numbers of the cottony-cushion scale and then migrated en masse to citrus trees bearing DDT residues which were still toxic and which killed them.

From these experiments, the Californian scientists deduced that a grove infested with cottony-cushion scale surrounded by orchards sprayed with DDT might be materially retarded in a clean-up by the natural enemies owing to the ' trap ' effect of the surrounding areas toxic to migrant adult predators or parasites. This ' trap ' effect was experimentally demonstrated in two groves where cottony-cushion scales had built up to serious proportions as a result of previous DDT treatment. At the time of the experiment the DDT residue had weathered so far that vedalia were expected soon to re-colonise the area from outside. In each orchard a number of trees were selected as test plots and two rows of trees surrounding the plots were treated with a single application of wettable DDT. Plots of untreated trees surrounded by untreated trees twenty rows distant served as check plots. The investigators found a lag in the increase of vedalia in the untreated area surrounded by DDT-treated trees and a less thorough reduction of the cottony-cushion scale, so that a definite ' trap ' action was demonstrated on the untreated area by the surrounding pesticide-treated trees.

When may such ' trap ' effect be expected to be of economic importance?

The ' trap ' can lead to a serious reduction of the population of natural enemies in areas where, first, a large percentage of the crops harbouring these natural enemies is sprayed, and, secondly, the spray residue is more toxic to the natural enemies than to the individuals of the pest arriving from adjacent unsprayed crops. Where such differential in toxicity exists and where the natural enemies act as an effective brake on the multiplication of a pest or of a potential pest, the use of non-selective pesticides should be regarded only as an interim remedy until selective insecticides are available. On the other hand, if the pest is as mobile as the natural enemy, a ' trap ' effect of the latter will not arise, and if the pest is more mobile than the natural enemy, the pest population on untreated plots will be lower than on untreated plots surrounded by untreated fields.

Long-term effects of pesticides on the balance of insect and mite populations are more obvious on perennial crops. But they occur also on annual crops if the sprayed area is a high percentage of that crop and the pests are oligophagous, that is feed only on a few plant species. When the pests are polyphagous, that is when they have a wide range of food plants, and the sprayed area is only a small percentage of an annual crop, only short-term effects of the pesticides are observed.

Reduction of the natural enemies by a chemical, that is a permanent lowering of the ' environmental resistance ' is a condition which needs attention because of the astronomical rate at which insects and mites under favourable climatic conditions increase in number when their natural enemies have been eliminated. May I recall that about one hundred years ago Huxley (1858) calculated that a single parthenogenetic female aphis could give rise in a year's time to a progeny whose total body weight would be equal to that represented by the inhabitants of the Chinese empire of his time. This was confirmed by Herrick (1926) who found the reproductive capacity of the cabbage aphid in a good year to lead to twelve generations between March 31 and August 15. A single stem-mother produced an average of forty-one young aphids; on this basis the progeny, if they all lived, would number over half a trillion. As the average weight of four aphids is 1·4 mg., the total number of aphids would weigh 250 million tons. These theoretical figures would seem to justify the attention given to the preservation of forces which prevent such a grotesque multiplication in nature.

Resurgences of the pest population after spray treatment can also result in an entirely different way, namely through favourable influences of pesticides on phytophagous insects and mites.

When the notorious increases of red

spider mite associated with DDT treatment were first noted in 1944 (Steiner, Summerland & Fahey), some authors at first thought them to be solely due to an elimination of the natural enemies of the red spider mites ; but it soon became obvious that DDT treatments produced an increase of mite population above that occurring on check trees—which was not explainable by a reduction of the natural enemies by the DDT.

Further light was shed on this in Professor Kuenen's laboratory at Leiden by an interesting investigation on the population dynamics of the fruit-tree red spider (Hueck, Kuenen, Boer & Jaeger-Draafsel, Hueck). Apple trees infested by red spider mites were sprayed with parathion. This killed all mites which were not in the egg stage, and all predators. After this application of parathion, a number of trees were sprayed with DDT suspension and some were left unsprayed ; for the following months the numbers of all stages of red spiders were counted every second day. An examination of the resulting data led Hueck *et al.* to the conclusion that the egg production had been substantially larger on DDT-sprayed trees than on the check trees. In similar laboratory experiments there was a significant increase of the egg production under the influence of DDT dust. The Dutch investigators did not study whether the stimulating factor travelled in the sap stream and their experiment was not designed to explore whether the stimulating factor acted as a direct influence of the spray chemical or via the plant, but Fleschner also obtained an indication that the factor which leads to the increase in the number of mite eggs moved into the sap stream forty-six days after DDT treatment but not before. He interpreted these and other experimental results as a lowering of the host plant resistance, but one can interpret Fleschner's results as an increase in the biotic potential of the spider mite.

III. Third Sequel

What influences on the balance of insect and mite population are caused through *removal of competition* amongst phytophagous species?

In contrast to the fruitful researches made into interspecies competition between pests of stored products, such as grain in silos, etc., there is little information on the extent to which plant-feeding insects and mites compete for their food supply, and very few references to the reaction of anthropod populations when competition is removed by pesticides. The evidence available is too scanty to assess the influence of the removal of competition through pesticides on the balance of insect and mite population and whether this factor may cause a resurgence of potential pests after chemical control of these phytophagous species which dominate the habitat.

The third important sequel to pesticide treatment is the segregation of resistant strains, and there has been more general discussion of this than any other. Out of 5,000 species of economically important insects, twenth-three, i.e. 0·5 per cent., according to Metcalf have formed resistant strains.

Resistance results from the selection of the most resistant individuals in a population by the pesticide, i.e. the chemical eliminates all individuals but genotypic variants which happen to be resistant. Resistant survivors of pesticide treatment must therefore be the target of any attempt to prevent this undesirable phenomenon. Mixtures of pesticides with two independent uncorrelated modes of action have been suggested (Hewlett & Plackett) as a possible method. This is analogous to the mixture of antibiotics to prevent resistance of bacteria from developing too rapidly, and also bears comparison with the alternation of weedkillers practised in order to prevent the substitution of susceptible species of weed by others which do not succumb to the weedkiller. But the analogy with antibiotics and the rotation of weedkillers is only superficial because there is no parallel for the repercussion of the pesticide on the natural enemies of the uneconomic organism.

Using the knowledge of cross tolerances of resistant insect strains as a guide, experiments are in progress to prevent the development of resistance by mixtures of pesticides with independent uncorrelated modes of action and also by the employment of alternative treatments with such pesticides. This method has, in our opinion, its place in habitats where at the

time of application no natural enemies of the pest are present which form an effective brake on the population density of the pest, and possibly also on annual crops where the area to be sprayed is only a small percentage of the total acreage under such crop and where the pest is polyphagous, so that the treated pest population form only a small percentage of total population present in the district. Where natural enemies keep the plant-eating insects and mites in check for part of the time on perennial or annual crops, and where a large percentage of the acreage is sprayed and the majority of the population of the pest in the district is thus involved, the application of mixtures of two non-selective pesticides will decrease the biotic resistance which might produce other sequels than the segregation of resistant strains.

IV. Prevention of Undesirable Sequels

How can we prevent unfavourable effects by pesticides on the balance of insect and mite population ?

(1) In some cases it has proved possible to minimise the damage to the natural enemies by timing pesticidal sprays before the majority of the predators and parasites become active, but the scope of this method is limited.

(2) Sometimes it is possible to minimise adverse effects on the balance of insect and mite population by the choice of the pesticide least toxic to the natural enemies. Several investigators have made great progress with this approach, which has been pioneered by Pickett and his collaborators in Canada, by DeBach & Bartlett in U.S.A., and by Collyer & Kirby in this country.

By finding out which insects are the most effective natural enemies of the pests and by adapting ecological methods to chemical control studies which clarify the direct and indirect influence of the spray programme on the fauna, a number of spray schedules were worked out and tried over a number of years. Thus resurgences of the oystershell scale, the European red mite and the eye-spotted bud moth were avoided by substituting sulphur applied against apple scab with copper and ferbam, glyoxalidine and captan (Lord, MacPhee, German, Clancy and lately Collyer & Kirby).

Our most promising approach to prevent undesirable sequels lies in the shift of the balance of arthropod population in favour of the natural enemies by the use of selective pesticides.

Although selective toxicity has dominated chemotherapy since Ehrlich's work in the first decade of our century, the conception of selective toxicity in entomology seems to have been formulated in 1944. A selective insecticide was defined as a chemical which kills the uneconomic insect and mite species, that is the pest, and spares the economic species, namely the pest's natural enemies. As a first example, we used the selectivity of nicotine vapour at short exposures which produce a high mortality of the cabbage aphid (*Brevicoryne brassicae*), while the predators of this pest and the parasite (*Aphidius brassicae*) were not killed.

In 1949 the exciting discovery was made that the systemic insecticide schradan was toxic to certain plant-feeding insects and mites such as aphids, whiteflies, scale insects and a number of spider mites, but did not kill their predators, nor the parasite *Aphidius brassicae*. Schradan did not kill Anthocorids, Mirids or Capsids against which it was tried. It kept aphid populations low for more than forty days, while the non-selective organo-phosphorous pesticides, parathion and paraoxon, produced a rapid resurgence of the pest population within fourteen days.

By the use of this selective insecticide, the balance can be shifted so much in favour of the natural enemies that they spill over into adjacent untreated pest populations and effect a biological control which is triggered off by the selective chemical used on the neighbouring treated area. A heavily blackfly-infested sugar beet field was divided experimentally into plots of 40 ft. width and the alternate plots were sprayed with schradan; but the aphids were controlled over the whole area. Within three days after the complete elimination of aphids on the sprayed plots, all the predatory ladybirds had moved to the untreated plots ; there they cleared up all the aphids within the next four days, so that on both sprayed and unsprayed plots the aphid outbreak

disappeared within ten days while outside of the experimental area the infestation continued.

As schradan does not kill arthropods by contact with their cuticles nor predators by ingestion of aphids killed by schradan, this type of selective action has been termed physiological selectivity.

Schradan is used in Britain on a field scale against aphid outbreaks on annual crops and is accepted as regular treatment on certain permanent crops. Over 90 per cent. of the British hop acreage has for the last six years been treated with schradan so successfully as to allow the reduction in the number of pesticide treatments required to control the hop-damson aphid (*Phorodon humuli*) and the hop red spider mite (*Tetranychus urticae*).

By contrast, the systemic insecticide demeton has the undesirable attribute of ' contact toxicity ' to predators and parasites. Its effect on predators after ingestion of aphids killed by sucking on demeton-treated plants has also been investigated. Three species of Syrphidae were regularly killed and the susceptibility of five Coccinellid larvae tested ranged from 100 per cent. mortality in some species to 3·7 in others. Adult Coccinellids were not affected except one species (*Cycloneda sanguinea*) of which 50 per cent. were killed. Larvae of Chrysopa were not affected.

Equally, methyl demeton kills a high percentage of predators, i.e. Syrphids and ladybird larvae and parasites by contact as well as the aphids. Hence resurgences of blackfly and cabbage aphid populations occurred this year, frequently within fourteen to twenty days after methyl demeton application, and three weeks after demeton application.

We attempted by another way to produce physiological selectivity, namely by coating the particles of a non-selective insecticide with a material which eliminated the contact effect to non-plant-eating beneficial insects, while stomach toxicity for pests was released after chewing or digesting of the coating of the particles. Further work on this promising method is in progress.

Where physiological selectivity cannot be brought into play, it is sometimes possible to exploit differences in the ecology of the pest and the natural enemies. Thus we were able to convert non-selective systemics which are well translocated in the plant's sap stream, into insecticides with ecological selectivity. Dimefox (coded in 1951 as CR 409) was found toxic to the cabbage aphid and killed by contact its natural enemies, but when applied to the roots of brassicas, produced a heavy mortality among the cabbage aphid without affecting the natural enemies. In a similar way, a high mortality of the hop red spider (*Tetranychus urticae*) was obtained by a soil treatment around the hop plant with dimefox without affecting the predator. We also showed that the coffee mealybug can be killed by a soil treatment without affecting its principal natural enemy. After the application, systemic insecticides are translocated to organs of the plant where the natural enemies are not killed because they never come in contact with the systemic insecticide.

The use of selective insecticides also seems to form a practical way to prevent resistant survivors of pesticide applications from multiplying because survivors are eaten by the predators and parasites whose food supply has been drastically reduced by the action of the selective pesticide. This hypothesis of prevention of the development of resistance occurred to us when studying the fate of survivors after a selective chemical treatment. The surviving hoverfly larvae concentrated for three days after treatment on devouring the survivors of the cabbage aphid population, then left when the population density of the aphid became too low, while the parasite (*Aphidius brassicae*) continued its parasitism until 100 per cent. of the survivors were parasitised and all survivors had been cleaned up.

While the possibility of preventing resistant strains by the use of selective pesticides has not been completely proved in clear-cut experiments, no case of a resistant strain being segregated after the use of a selective pesticide has been recorded.

V. Discussion

From this discussion it will be seen that ecological concepts and methods have served applied biology well.

A discussion of the influence of pesticides on the balance of arthropod population would be incomplete without a few remarks on the application of this data to practical problems of control of agricultural pests.

We have seen undesirable sequels result through a destruction of the natural enemies, hence non-selective insecticides should only be used after one has made certain that they do not increase the reproduction of pests or potential pests, and they should be applied only under conditions where pests are present without any effective natural enemies and without any other potential pests occurring which are at the time controlled by the natural enemies.

Where natural enemies are present which can, unaided, for part of the time or with the aid of chemical treatment, control a pest or a potential pest, then selective insecticides or non-selective insecticides used in a way which avoids unfavourable influence on the natural enemies and stimulation of the phytophagous species, should succeed in keeping crop protection costs at a minimum and avoid the disastrous sequels which I have described.

The satisfactory performance of selective pesticides justifies a far wider study of selective toxicity differentiating between economic and uneconomic insects and mites.

The method of combating resurgences of ' man-made ' pests by adding a second pesticide which is not selective although natural enemies are present, can safely be practised only in cases where the sprayed area constitutes a low percentage of the crop in the district, or where there is thus a large population of the same pest species on food plants outside the sprayed area.

To prevent the development of resistant strains, the use of a mixture of two non-selective pesticides of independent uncorrelated action or alternate treatments with such materials is being investigated. But even if successful, its impact on the balance of insect and mite population would again only be applicable where no natural enemies of the pest or potential pest are present. Where natural enemies occur, the use of selective insecticides is far more promising and has so far not led to a segregation of resistant strains.

It will take time to develop selective pesticides or spray programmes compatible with biological control for all those pests where effective natural enemies are at work or are available. Against serious pests for which selective pesticides are not yet available, the use of non-selective pesticides will therefore in the meantime have to continue ; for habitats where natural enemies are present, this must be regarded as an interim solution which is becoming steadily more costly and dangerous.

As this story unfolded we have seen cases where the application of fungicides by plant pathologists interferes with the natural enemies of pests against which the entomologists were trying to find further chemical treatment, and the same is true for nutrients.

It might therefore be well to consider what steps should be taken to co-ordinate the effects of the various specialists working on one crop so that the effects produced do not clash and offset each other.

A big research programme is involved in the reorientation of crop protection towards a combination of biological and chemical control. This might call for additional appropriation in Government budgets for applied biologists in this country and overseas, but the financial savings to farmers and growers from crop protection by chemicals are so big that such an investment seems justified. Research might also be assisted by a better division of labour between industrial and official research workers. The job of scientists working on pesticides in industry is to produce new molecules of high biological activity, to screen them for desirable and the absence of undesirable effects, and then to offer them to Government research workers who are concerned with the solution of crop protection problems. They in turn try the various promising molecules and decide which would form the best treatment for a given case. The research workers would present their recommendations to a co-ordinating officer for each crop, who would, in consultation with these specialist officers, agree a treatment which seemed to be the best all round.

Summing up, a study of influences of pesticides on the balance of insect and mite population shows that we are right to consider a growing crop as a biological system designed to produce a maximum of food for humans, which often has as a defence system against plant-feeding pests, a population of natural enemies of the pest.

Our study has shown that the undesirable sequels of pesticide population in the short-term resurgences and long-term effects including segregation of resistant strains, do not arise if the chemicals used to correct pest infestations are so chosen as to eliminate only the offending pest species, and thus supplement the defence system, not destroy it.

If I may put it in the concise way which we owe to the Canadian entomologist, A. W. A. Brown :

> 'In crop protection the chemical weapon must be used as a stiletto, not a scythe.'

THE WEST COUNTRY: SOME HISTORICAL CONTRIBUTIONS TO SCIENCE[1]

by

C. L. BOLTZ

THOUGH the term 'West Country' has been in use, according to the *Oxford English Dictionary*, for nearly six centuries, nobody has defined it. Needing limitations to this short paper I have chosen six counties—Gloucestershire, Wiltshire, Dorsetshire, Somersetshire, Devonshire and Cornwall—and in time stopped short of the middle of the nineteenth century, thus avoiding far less than justice to Bristol University and its forerunners.

In my period the six counties have given a baker's dozen of scientists of importance (with an additional West-Country worthy to be added as a tribute, though he died only three years ago).

There are two natural philosophers. Five are medical men. Three are engineers. One is an astronomer and nothing else. One is a chemist. One is difficult to classify, for though everyone knows of Wren as an architect how many remember that he was Savilian Professor of Astronomy at Oxford for a dozen years?

First in order comes Roger Bacon. He was born in 1214 at Ilchester in Somerset and came of a well-to-do family. It is well known that he became a scholar and a Franciscan friar.

Roger Bacon is the most controversial of the people on my list. To some he is the first of the modern scientists, advocating observation and experiment instead of disputation and servitude to the syllogism. To others he is much overrated, merely a mediaeval writer of confused ideas. According to one modern historian of science, Bacon was at Oxford a pupil of Robert Grosseteste, whereas according to the member of the Dominican order who wrote the article for the modern *Chambers's Encyclopedia*, he was certainly *not* a pupil of Grosseteste. (This same writer, incidentally, puts Ilchester in Dorset.) To some Bacon was, if not the inventor, certainly the first chronicler, of gunpowder. To others this is merely part of the Bacon myth. *This* Bacon, *Roger* Bacon, is also thought by some to have used secret codes in his writing to put on record facts—such as the proportion of the ingredients of gunpowder—which he did not want made public.

Among all this clash of opinion, *modern* opinion, the facts we can state without fear of contradiction are few, but they are enough to place him high in importance in the history of science. Roger Bacon was part of a wonderful flowering of independent and challenging opinion that shook authorities in the thirteenth century.

The Crusades, not yet over, had brought the warring West into touch with the learning of the East. Through the Arab custodians of knowledge Europeans of the Middle Ages learned of ancient science and mathematics. And Western Europe was a cultural whole, so that Norman and Provençal argued with Englishmen at Oxford, while students from Britain walked and rode to Paris, there to sit at the feet of masters of learning. Latin was the universal language. An intellectual ferment threatened the Church; the University democracies threatened feudalism.

Roger Bacon was a part of all this. Nobody has yet traced *exactly* when he was at Oxford and when in Paris. What is certain is that he had long periods at both. It is also certain that he sent to Pope Clement IV, a Frenchman who had been Papal Legate in England, treatises that were intended to be a whole review of knowledge. Some manuscript has even been found in the Vatican archives. It was, we are told, the Pope's own wish that

[1] Address delivered to the Assembly of Corresponding Societies at Bristol on September 5, 1955.

Bacon should send accounts of the sciences.

Bacon did original work in optics. Even his modern critics admit this. He worked on mathematics, especially geometry. His imagination leaped ahead to conceive of aeroplanes and motor-cars. He wrote on astronomy, the reformation of the calendar, and agriculture.

Positive writings on science combine with Bacon's trenchant criticisms of scholastic disputation as a means of arriving at truth to mark him an important mediaeval influence in the growth of experimental science. We need not worship unduly as some did half a century ago. We need not go to the other extreme and debunk him. We need not be so pedantic that we leave him drily and non-committally on one side. Perhaps the greatest work on Bacon is yet to be written ; the writer will have to be a very considerable scholar to do it. All we need do is to welcome him in science as the first of West-Country worthies.

Next we come to a group of five medical men. The first, Francis Glisson, was born in 1597. The last, Edward Jenner, died in 1823. So the five span two centuries, though four of them were more or less contemporary.

Glisson, who came from Dorsetshire, was the first physician ever to produce in Britain a monograph on disease. This disease was rickets, and his observation and description have never been bettered even today. If you want to know what causes rickets and how to prevent it, you will not go to Glisson. But if you want to know what the disease is like in all manifestations, Francis Glisson is still your man. He produced also a book on the anatomy of the liver, and the fibrous sheath of this organ is now always referred to as Glisson's capsule. When he died at the age of 80 he had just published his third book, a treatise on the belly and the intestines—no bad record for a West-Countryman born less than ten years after the incident of the Spanish Armada.

Thomas Willis was the son of a Wiltshire farmer. His name is perpetuated in the *Circle of Willis*, a name given to the arrangement of arteries at the base of the brain. It was described in his book, *Cerebri Anatome* published in 1664. Many of the illustrations in this book were drawn by his friend Christopher Wren. He was the first to describe and name puerperal fever and diabetes. He was the foremost physician of his time in London. He became Court Physician to Charles II.

It is notable that both these eminent physicians were concerned with description. It was part of the scientific revolution that accurate description and illustration were essential preliminaries to the understanding of function. It is a tribute to Glisson and Willis that the name of each should go on in text-books because of the accuracy of their observation and the care in their descriptions.

We come to the third of our group of four contemporary medical men. Thomas Sydenham was the son of a Dorsetshire landed proprietor. His young manhood was taken up by war. Only after the defeat of Charles by Cromwell at the Battle of Worcester was Sydenham able to settle down into civilian life. He started in practice as a physician in London.

The interesting point about all this is that he was not really legally qualified to practise. In point of fact he was nearly forty before he received the licentiate of the Royal College of Physicians in 1663, and he was fifty-two when he took his University Doctorate in Medicine at Cambridge.

All these facts are important in our understanding of Sydenham's career. He was obviously not an academic, not a theoretician. He was a healer, a practical man who had seen life and death under the worst conditions. A doctor, he seems to say, should learn diseases, study the conditions that cause them, know the way to treat them. He first expressed the opinion, so commonplace today, that the symptoms of a disease are the expression of a struggle between the body and the disease-causing agency.

Sydenham's emphasis is on the clinical. He observed endlessly, not dissected bodies or parts of bodies, but sick human beings, and so learned to recognise specific diseases. He gives admirable descriptions of smallpox, measles, dysentery and syphilis. It is important for us to realise that his emphasis was not the usual one. In the days of old, Hippocrates had

treated each illness as an individual thing unlikely to be seen again, whereas Sydenham, often called the English Hippocrates, though reverting to clinical practice, recognised the reappearance of certain symptoms and so could classify them and establish specific groups of symptoms, each group being special to the disease.

From this came diagnosis as a physician's skill. You looked, you tested, you remembered, you classified, you eliminated. In the end you arrived at a decision; the appropriate treatment, as known at the time, then followed. It is a *modern* outlook.

All this achievement—and Sydenham was as famous throughout Europe as he was in England—came when nobody had any clear idea of why we need food and drink, why a living body should be warm and a dead one cold, or what the process of respiration is.

In elucidating some of these questions the next one on our list contributed a great deal. Richard Lower was a Cornishman, born near Bodmin. His beginnings were what we should call orthodox today —Public School (Westminster) and University (Oxford). He took his B.A. degree when he was twenty-two. As orthodox a nurture as you could wish for. And he began to study medicine with another West Countryman I have already mentioned, Thomas Willis, who was then Professor of Natural Philosophy.

Lower became a highly skilled anatomist to whom his contemporaries paid tribute. He gained his Doctorate in Medicine in 1665, when he was thirty-four. In that year he did something that has come down to modern times as an outstanding physiological experiment. And he did it at the suggestion of no other than Christopher Wren, who we now find applying himself to physiological speculations. This suggestion was that blood, being a circulating fluid, the conveyor, perhaps, of health, could be transferred from one animal to another. Wren suggested it; Lower did it, with dogs, in 1665, the very first blood transfusion. Two years later he transfused sheep's blood into a man.

In 1669 he produced a now famous book on blood circulation and the heart: *Tractatus de Corde; item de Motu et Colore Sanguinis*. It is the thought and observation in this that is Lower's chief claim to fame, for he tackles problems of fundamental physiology about which nearly nothing was known. Harvey had, it is true, established the circulation of the blood, but neither he nor anyone else could explain how the blood got from the arteries to the veins in the tissues of the body. By the use of injections Lower decided that the veins and arteries must end in fine hair-like branching tubes, leading to still finer ones invisible to the eye, and it was these invisible capillaries that communicated with each other so that blood could get from the outward circulation to the inward circulation.

It was generally believed that the heart moved because of a frothing of the blood contained in it, but Lower showed that the heart wall is a muscle and so could contract like other muscles. This contraction made the pumping action of the heart.

On the subject of respiration Lower said that the object of respiration was to bring air near the blood, and so make it suitable for the heart. This condition was, he said, indicated by its becoming bright red.

All these discoveries were original. They mark Lower as a physiologist of importance.

Now we must jump almost a century. The last of our famous physicians of the West Country is Edward Jenner, the first on our list to have come from Gloucestershire. His father was a parson and Edward Jenner was first apprenticed to a surgeon and then sent to London as a pupil resident in the house of John Hunter. He did his professional studies at St. George's Hospital.

At the age of twenty-four he was back in his native village of Berkeley practising as a physician. And there he stayed for the rest of his long life—fifty years as a country doctor, famous locally for his blue coat and topboots with silver spurs.

He was a naturalist, a geologist. He collected specimens and described them. He produced one booklet called *The Natural History of the Cuckoo*. He formed a medical society at Rodborough and belonged to another that used to meet at the Ship Inn at Alveston, not far from

Bristol. He was elected a Fellow of the Royal Society when he was thirty-nine.

The greatest thing he is noted for you all know as well as I do. He established vaccination as a preventive treatment against small-pox. It was the beginning of all inoculative prophylaxis, though nothing was known at the time about the germ theory of disease.

The honours that poured on him are too many to mention. He was, in any modern sense, the most renowned of all our medical West Countrymen.

And now let me jump back the century to the one whose position in science I haven't been able to decide. I mean Christopher Wren. He was a genius. You may think of him as an architect, but he had to start learning architecture at the age of thirty. You have already heard how he illustrated Willis's book with exquisite drawings and how he suggested blood transfusion to Lower. These are but aspects of his versatility. The illustrations to Willis's book were done when he was twenty-one. He invented a planting machine for farmers and then went on to explain and illustrate a scheme for the graphical construction of solar and lunar eclipses. When he was twenty-four he solved a problem proposed by Pascal to the geometers of England and retorted by setting one himself for French savants. His challenge was not answered.

He produced four tracts on the cycloid, and Newton had a very high opinion of his mathematical work. Wren then showed the connection between barometers and the weather and suggested a scheme for purifying and fumigating sick-rooms. Nothing was out of his range, and he could accurately be described as much a famous meteorologist as architect.

We now come to a group of very different practitioners in science, the engineers.

The first of them was Thomas Savery, a man of Devon. Very little is known of his early life, though he has been described as a military engineer with the rank of 'trench master.' But such a rank did not exist in the seventeenth century. What is certain is that he was not of really humble origin as has sometimes been said; in his patents he is described as 'gentleman.' What we do know about him with certainty is that he was an inventor. He was for ever inventing. And he asked for patents on some of his inventions.

It was a time of invention. New England, with the National Debt and the Bank of England established and even the beginnings of cabinet government, as she emerged from the fifty years of struggle, was an England of unrestricted commercial competition. Everywhere there was the same spirit, wanting to break with the old and get on with the new, which was enterprise and trade and material advance. Companies were formed for trading all over the world. So many applications for patents had never before been received by the Crown—you can read of them in the Calendars of State Papers—ranging from street lighting to the organisation of collections for charities.

Savery was part of all this. In a pamphlet called *Navigation Improv'd* he expounded a new method of propulsion of ships. He argued from the principle of the lever that the longer the arm of the oar between the oarsman and the rowlock and the shorter the arm between the rowlock and the blade, the greater was the propulsive force for the same expenditure of manual effort. And, obviously enough, a dozen oars were better than one. But if the oar was made too long it was too heavy and a man would not pull it. So why not combine a short rowlock-to-blade distance with a long rowlock-to-oarsman distance by transferring the motion from a horizontal one to a vertical one? In other words, he arranged a number of short-hafted blades round a horizontal shaft and rotated this shaft by means of a capstan turned by two men with very long poles. As you can see, if you have followed this closely, what he contrived was a paddle-wheel propulsion operated by men. Two men could thus effect as much as a dozen conventional oarsmen.

The invention for which he is best known and with which his name is always associated is a device utilising steam to raise water. He took out a patent in 1698 and successfully demonstrated a model to the Royal Society a year later. He had some knowledge of the effect of advertisement and was able to persuade important people to listen to him. Three years later he tried the machine out on the tin miners of Cornwall in a pamphlet called *The Miner's Friend.*

The principle of the device is simple enough. First of all you have a boiler to raise steam in. Then you have some sort of receptacle—illustrations show it as a sort of double-ended flask. This is attached at the lower end to a pipe dipping down into the water to be raised. The upper end is attached to a pipe going vertically upwards. You let steam force itself into this receptacle, driving out the air, while the bottom opening is closed. Then you close the top, open the bottom, and pour cold water on the flask. The steam condenses, forming a vacuum, and the water below rushes up into the flask. You then close the connection to the lower pipe, open the top one and let more steam in. The pressure of this drives the water up through the vertical pipe. Thus you raise the water from one level to another.

One of these steam engines, so called, was made for a house in Kensington, London. It cost £50 and raised 3,000 gallons of water an hour. It used a bushel of coal a day.

Despite the ingenuity of the device—it was the first steam operated water pump—Savery did not succeed. The tin-miner did not want his engine. Nor did many householders. The machine was not efficient and it needed men for doing the monotonous business of turning on and off cocks all day long.

Only a very few years later than Savery came Thomas Newcomen, of more or less the same generation in fact. He, like Savery, was a Devon man. He was, we are told, an ironmonger and blacksmith in Dartmouth. Savery was working only 15 miles away and it is reasonable to suppose that Newcomen, who was, it seems, a highly skilled person, made some of the more intricate parts of Savery's engine. In fact, we know as little of Newcomen's beginnings as of Savery's, though it is evident that Newcomen's family had been some time in the area and was respected.

He began to think out the various ways in which he could utilise steam from coal in a more convenient way. Papin had been trying to arrive at a way of transmitting steam power over a distance to operate mechanisms, and Newcomen toyed with this. It is said that Robert Hooke, that practical genius of the early Royal Society—*almost* a West Countryman, by the way, for he came from the Isle of Wight—threw out the suggestion that if Newcomen could get the movement directly on to a piston, that was the solution.

This is just what Newcomen did. Gone was the fixed receptacle of Savery. Instead the receptacle became a cylinder and the top part a piston moving in the cylinder. When steam was injected into the cylinder and then condensed by cooling, the atmospheric pressure on top of the piston moved it downwards. By successive injections and condensations of steam it was thus possible to get an up and down movement of the piston. In this we begin to see something that is recognisably a steam engine. The work was done largely by pressure of the atmosphere and so it was called the *atmospheric* engine.

The transmission of motion from the piston to a pump shaft was done through an overhead beam. This type of beam atmospheric steam engine is familiar I expect to everyone.

There were always difficulties with these early engines, for machining was not what it is today and there was also a considerable difficulty in getting an airtight connection between the piston and the cylinder wall. Newcomen used water resting on the piston to get this airtight connection. This method was used in the first Newcomen beam engine built. It was in a mine near Wolverhampton. Newcomen had difficulty in making the engine go. Then there was an accidental discovery. The engine was seen to go quite rapidly for a few strokes and then slow down. The cause of this, they found, was a small hole in the piston. This allowed cold water to get through the piston into the cylinder below, and Newcomen realised quickly that this accident could lead to a greatly improved engine. So he arranged for an injection pipe to go into the cylinder. Through this pipe cold water could be injected straight into the steam and so condense it very quickly.

This Newcomen beam engine was a practical thing. Newcomen's second and third were built at Newcastle upon Tyne, his fourth at Leeds, and his fifth at a

Cornish tin-mine. The pumping situation in many of the deeper mines in Cornwall was pretty desperate and Newcomen engines grew in demand. They went on doing their stuff for half a century and more after they were erected.

The development that killed them came from James Watt. It was he who realised the importance of a separate condenser so that the cylinder would not get cold. This was his master discovery.

I bring in Watt here to show the line of development and get the time-sequence in order. My business is not to trace the detailed history of the steam engine, but to show what part West Countrymen played in its development. Watt was, by the way, completely the opposite of Savery and Newcomen. Watt was a thinker, a brooder on fundamental phenomena. Only the combination with Boulton made Watt successful. But for this partnership he might have remained unknown, an unhonoured experimenter.

Condensation in a separate vessel, and the use of the expansion of steam, so that it could be turned off before the piston stopped movement ; these were the two major changes that gave greater efficiency and made the tin-miners take to the Watt engine.

The first installation of a Watt engine in the Cornish mines took place in 1777. This date is relevant. So is another fact. Boulton and Watt did not sell engines, but demanded instead a royalty payment based on how much more efficient that engine was compared with a Newcomen engine. This payment was presumably to go on for ever.

When their first, very successful, pumping engine was built in Cornwall in 1777, there was a boy of six years old not far away who was destined to be a considerable rival to them. This boy was Richard Trevithick.

The story of Richard Trevithick has not been fully told in modern terms. It is a story of success and failure, of high character and romance and adventure.

Richard Trevithick's father was the manager of a tin-mine. The boy went to school at Camborne. It was all the schooling he ever had. As he grew up he had plenty of opportunity of helping with the engines used in the mines and of hearing the arguments and complaints about the lifelong payment to ' foreigners ' like Boulton and Watt.

He grew up to immense strength. He was 6 ft. 2 ins. high, with immense broad shoulders. He could lift half a ton unaided—the metal he lifted to astound his fellows is now at the Science Museum in London. He was a champion wrestler of the West-Country style. When other men competed to get a heavy sledge-hammer nearest to a wall of a house, Trevithick threw it over the very house itself.

At the same time his face was round and open, his blue eyes alight with friendliness and enthusiasm. He was a gangling giant of a Cornishman, impetuous and imprudent. And he was full of inventive ideas ; a ' volcano ' of ideas, is what W. H. Dickinson has called him.

He was fortunate in making friends with Davies Giddy, afterwards called Davies Gilbert. Giddy was a West Countryman, too, and would have deserved a place in our list had he really been a scientist. But he was more a literary man, and though he was later President of the Royal Society, he was a Fellow more for his patronage of promising men, whether scientists or not, and his general promotion of learning. Giddy was a friend of Trevithick's for the rest of his life. His help came in two main ways. First there was his friendship with influential people, even to the highest in the land. Secondly, there was his training to sieve the rough ideas that came to Trevithick and see whether they were worth following up.

Trevithick worked as a sort of engineer to see to the pumping engines, and he was always suggesting practical improvements whether on the Watt engines or old Newcomen engines. The chief purpose of improvement was to economise in fuel. The practical problem then was to get as much energy as possible from every pound of coal, just as today the chief preoccupation of every nuclear physicist or engineer concerned with power reactors is to get the greatest possible amount of energy from every gramme of Uranium 235 or plutonium—the variety of designs of reactors discussed are signs of the attempts now being made to achieve high efficiency.

Trevithick realised in practice an effective plunger pump as it was called, or force pump as we should call it, to get water up from great depths. He invented a water-pressure engine, that is to say, a pump whose shaft was made to move by water-pressure, and in his double-acting water-pressure engine he achieved the best pump ever made to work on the vacuum principle.

Another idea was constantly occupying his thoughts. It was that of using steam at high pressure. Watt was afraid of using any pressure higher than atmospheric. Trevithick was not afraid ; he was adventurous. In 1800 the Watt patent expired and in that year Trevithick built a high-pressure steam engine and put a crank on it and fitted it in a tin-mine.

The principle of this was not that of condensing steam to make a vacuum. It was the principle of using steam at high pressure to push the piston and then escape into the air. This noisy rush of gas into the air at every stroke led to the local use of the word ' puffer ' to describe the new engine. This was, I believe, the first use of this word for this purpose.

High pressures brought a reduction in size of cylinder and stroke, so that the new type of steam engine, dating from 1800, is recognisably a steam engine as we know it today, whereas all the old condensing engines seem to us archaic devices. There are people alive today who could have seen Trevithick engines working. They were in some demand, with the crank action, for other purposes than pumping.

For Trevithick, one other purpose was locomotion. In 1801 he built an engine and connected wheels, and on Christmas Eve of that year the new monster took the road and careered about Camborne giving people lifts. It was known as *Captain Dick's Puffer*. It was not the first steam locomotive ever, for two others had been tried, one by a French engineer and one by William Murdock, without much success. Trevithick's locomotive, though it met with mishaps, was the first that can be called a steam locomotive as we understand the term today.

In the following year, 1802, Trevithick and Giddy took out a patent for the high-pressure engine.

A year later another steam carriage was shown in London. The element of sport now comes into the picture, for the manager of an ironworks in Wales commissioned a steam carriage to work on existing rails and was bet 500 guineas by a friend that it would not haul 10 tons of iron along the 10 miles of track. But it did, so the scoffer lost his bet. Some years later a sporting challenge was made by Trevithick that a steam carriage would compete with a racehorse at Newmarket, but the challenge was never taken up.

The locomotive on wheels did its work, but smashed up the rails. Nobody just then seems to have realised how near Trevithick and his friends were to becoming the pioneers of our railways. (Incidentally numbers of Trevithick's descendants have held high posts in our railways.) But because of difficulties and perhaps the impetuosity of Trevithick, he left steam locomotion alone and went on to other things.

His stationary engines were in demand, but one exploded in 1803 and Trevithick's stock was low, despite the fact that he immediately set to work to improve the design so that it could never happen again. That it happened when it did, we are told, was due entirely to the disobeying of instructions by the man in charge of the engine.

All the time the design was improving, but he did not limit himself to steam engines alone as such. He invented a dredger to deal with silting-up at the docks. He got more than three-quarters of the way across the Thames in a scheme to construct a tunnel. The promoters closed down the project, largely because of opposition from men who did not like Trevithick. He was in London by this time and next joined with a partner in a company to make iron tanks for ship's cargoes and for water. He advocated iron ships.

Then he went sick with typhus and collapsed physically and mentally. It took him months to recover. The following year his firm was bankrupt. Yet he had paid off sixteen shillings in the pound when he was discharged from bankruptcy in 1814, at the age of 43.

Back at his pumping engines, his many improvements in the steam engine showed up together. The water in the boiler was

directly heated by a pipe from the fire, a forerunner of today's steam-engine boiler. The steam was cut off before the end of the stroke so that the expansion could do work. And—the sporting interest again—he offered a thousand pounds to five hundred as a bet against a rival engine for pumping in the mines.

And now, to cut a long story short, comes the most extraordinary part of it. His engines were taken up by a Peruvian mining company and in 1816 Trevithick left everything and went off to Peru himself. He got involved in the Civil War, fought with Bolivar, was rescued from death by drowning by a Venezuelan, made a handsome sum from some copper mines and lost it all on a speculation in pearl fishing, tramped across the Andes almost starving and quite penniless, was loaned fifty pounds by the son of George Stephenson and returned to Falmouth in 1827, having been abroad eleven years.

He went to Holland to look at pumping engines. He had ideas, sound ideas, for refrigeration, for the screw propulsion of ships, for a gun-carriage, even for a fantastic and tremendous iron column a thousand feet high in London to commemorate the Reform Bill. He was working on some of his ideas in the workshop of another man in Dartford when he died and was saved from a pauper's grave only by the subscriptions of his fellow workmen.

Trevithick has been called the greatest inventor who ever lived. Had he been thrifty and followed up only a few of his ideas in a persistent and efficient way, he could have been rich and honoured. But he would not, of course, have been Richard Trevithick who was as free with his money as he was with his genius. The pity of it is that the money was not enough whereas the genius was never exhausted, not even at the time of his death.

Now we come to as great a contrast to Trevithick as can be imagined, and the only practising astrononer in our list. James Bradley came of a pretty well-to-do family in Gloucestershire, but he spent a lot of his boyhood with an uncle living at Wanstead in Essex. This uncle was a clergyman and a well-known amateur astronomer. So while still a boy Bradley was quite familiar with the best astronomical practice of his time. He went to Oxford and then returned to help in his uncle's observations. He was elected a Fellow of the Royal Society in 1718, when he was twenty-five years old, at the time when Newton was president. In one of his first communications to the Society he described his early attempt to make an automatic device on a telescope in order that it should remain trained on a heavenly body.

His first original contribution to astronomy was the announcement of an error in observation of stars. He is said to have jumped at the explanation when sailing on the Thames.

The ship's burgee did not show the true direction of the wind—it never does. The realisation that the way it pointed was the result of combining the ship's direction of motion with the true wind direction gave him an idea. In this way he came, after a year of observations, to the phenomenon of aberration of light, which he attributed to the earth's motion in its orbit. His calculation of the amount of aberration was very little different from the value accepted today.

His second discovery was the fact that the earth's axis ' nods ' as it were, and for this he was awarded the Copley medal of the Royal Society. His conclusion was the result of twenty years of observation. He was by this time astronomer-royal. His achievement was to point the way to really accurate plotting of the stars and planets. He put real exactitude into astronomy.

The twelfth man on my list is Thomas Young. He was born at Milverton in Somerset, the son of Quaker parents. His father was both a clothmaker and a banker. There was no lack of money.

Thomas Young was precocious. At the age of two he could read fluently, and before he was five he had memorised the whole of Goldsmith's *Deserted Village*. He started Latin before he was six.

The two main interests of his life were science and languages. When eight years old he found a *Dictionary of Arts and Sciences*, which he began to read ' with the most intense interest and delight,' to use his own words. At school he came under the influence of the usher, who lent the boy Martin's *Lectures on Natural Philosophy*.

He was particularly interested in the part of the book that dealt with optics, and with the help of the usher he learned to use a lathe and make telescopes. At the age of thirteen he spent his pocket-money on Greek and Latin books and a Hebrew Bible. Later he added Italian to the languages he knew. At home he made a miscroscope and studied plants, and borrowed Hebrew, Chaldean, Syriac, and Samaritan grammars. He had not yet turned fourteen.

He became a medical student and wrote a paper on the accommodation of the human eye, reasoning that the accommodation was managed by changes in the power of the lens. This was the first correct explanation, revived years later by Helmholtz. He was elected a Fellow of the Royal Society when he was twenty-one.

Young was unquestionably a genius. He excited everyone who met him. His mind ranged adventurously in the realms of knowledge and speculation. He aroused controversy. He was industrious to a degree unknown today, and indeed in any age, except in a very few people.

He laid the foundations of physiological optics by experiments he performed on himself. He invented an instrument for measuring the eye's accommodation. He delivered the Bakerian lecture to the Royal Society *On the Theory of Light and Colours*, in 1801, when he was twenty-eight. In this paper he stated his belief that we see colours by means of a three-fold mechanism in the eye. This theory of colour-vision is, of course, the trichromatic theory, sometimes known as the Young-Helmholtz trichomatic theory because Helmholtz revived the theory half a century after Young. This trichomatic theory is the most widely held theory today and is the basis of the best-known and most exact system of colour measurement.

He gave a series of lectures on Friday evenings at the Royal Institution on natural philosophy, ranging far and wide and throwing out fruitful ideas everywhere. His account of elasticity has given us the term *Young's Modulus*. His method of construction of light rays is used everywhere today.

At the same time he was developing his wave theory of light, using it to explain the phenomena of interference—Newton's rings and the colours of these films and so on. By his forthright and acid comments he had made an enemy of a young man who was later to rise to eminence as Lord Brougham. Brougham retorted by three articles in which he dismissed Young's papers as 'destitute of every species of merit.' His theories, said Brougham, were mere 'lucubrations.' These criticisms, in a style we fortunately never see today, so disgusted Young that he gave up physics for years. This Brougham, by the way, was eventually Lord Chancellor and the founder of London University.

We know now that Young was right in every single one of his ideas.

When away from physics Young set about another problem, the deciphering of the Rosetta Stone. This fragment of stone had been found by French troops at Rosetta in the Nile Delta and was by 1802 in the British Museum. The remarkable thing about this stone is that there are three lots of writing on it, one in Egyptian hieroglyphics, one in the popular demotic Egyptian script, and one in Greek. All three are considerably defective, for the stone is only a fragment.

It was obvious that if the three writings told the same story, then a translation of the Greek version would give a clue to the hieroglyphics and so open up an immense amount of Egyptian writing for the archaeologist and historian. It was to this task that Thomas Young brought his industry and knowledge of Middle-Eastern languages in the year 1814. In a few months he had completed a translation of the demotic writing, thus overtaking all previous efforts by others over several years. It was not perfect but it was the best so far achieved. In doing it he realised that some of the demotic script was degraded from hieroglyphics, to which he now gave his full attention. He never succeeded in deciphering all of it but he laid the foundations of Egyptology by his discovery that the hieroglyphics were phonetic. His great rival in the field was Champollion, who gave all his life to Egyptology, whereas Young gave only part of the time of a very few years to it. So without underrating Champollion

we can say that Young 'first penetrated the obscurity which had veiled for ages the hieroglyphics of Egypt.' These words are on his tomb in Westminster Abbey.

These major discoveries in physics and Egyptology are by no means his only contributions to scholarship. It would take hours to outline what he achieved. So I will leave it at that. It may well be the judgment of the future that Thomas Young was the greatest genius of science that the West Country has ever produced.

And so to my last worthy, Humphry Davy. He came of modest but well-established parents in Penzance, and like one or two others on our list he was early taken up by a friendly man who fostered the boy's abilities. He was a dreamy boy, given to solitary wanderings, the writing of verses, the study of natural history, something of an idle fellow until the death of his father brought him up against the problem of earning his living.

He was apprenticed to a surgeon, an occupation that gave him plenty of spare time for his hobbies. He wrote poetry like this :

> By the orient gleam
> Whitening the foam of the blue wave that breaks
> Around his granite feet, but dimly seen,
> Majestic Michael rises. His whose brow . . .

It was a style of writing popular at the time and long since out of fashion. It's difficult to realise that the young fellow who wrote this was subjecting himself to a Wellsian regime of self-education. Some chemical experiments with primitive apparatus were part of this regime.

The same man who was so important in Trevithick's life, Davies Giddy, now came into Davy's and had soon recommended him as a coming lad to Thomas Beddoes, a doctor who had set up in practice in Bristol and established what he called the *Pneumatic Medical Institution*, in which he hoped to show the virtues of various gases, especially carbon dioxide, in curing diseases. We may smile at this today, but it was not all that foolish at the time in the light of knowledge as it was then.

To this household in the Hotwells district came young Davy in 1798, when he was just twenty. He was a shy youth with no obvious attractions, but some quality of enthusiasm shone in his luminous eyes and some quality of youthfulness made people take to him. In the Bohemian ménage of Beddoes, with the frequent arrival of famous men in the world of science and the arts, Davy really began his scientific career. Beddoes, the enthusiast, was certain that he had acquired a genius.

But Davy did not take much to the treatment of patients. What he was concerned with was his experimentation. One of the gases he worked with was nitrous oxide. He took a great deal of trouble to purify this gas and then he breathed it. This was in 1799. He described the effects . . . ' this gas raised my pulse upwards of twenty strokes, made me dance about the laboratory as a madman, and has kept my spirits in a glow ever since.'

Thus laughing-gas was discovered, now the stock anaesthetic for dentists everywhere.

Davy showed great qualities as a chemical experimenter and an observer and careful chronicler. His fame was spread by Beddoes. His researches on nitrous oxide were published. He was offered a post at the newly established Royal Institution in London. There his very first lecture was a great success, and the shy country bumpkin, though somewhat polished perhaps by his sojourn in the Beddoes' household, became a social lion and never looked back. In about a year he was Professor of Chemistry. In 1803 he was elected a Fellow of the Royal Society.

At about this time Gillray produced a coarse cartoon depicting a lecture at the Royal Institution, the chief feature of which was a man inhaling laughing-gas and emitting it in enormous clouds that had ripped his breeches.

This, of course, for Humphry Davy, was fame. He went on from strength to strength in his researches. He prepared potassium and sodium for the first time and named them. He discovered chlorine as an element and named it. He produced his safety lamp. He made arc lamps. He tried an electrolytic device to stop the corrosion of ships, a method only now coming into its own. I expect you know the story as well as I do.

He was knighted. He was made President of the Royal Society. And then his greatest discovery lived on after him. I mean, of course, Michael Faraday.

Which brings me in 1829 to the end of my thirteen scientists of the West Country, all of them important, one or two of them geniuses, a list of which we can all be proud.

But I have not yet finished. I would like to go back on my original plan for one exceptional addition. I said that I would go no further than the middle of last century, but there is one whom no history of Bristol University would mention I feel sure. So there is some justification for a brief mention here. I mean Sir Richard Gregory.

He was born in Bristol. His father was a shoe-repairer and a pioneer of working-class education, a man of outstanding qualities. Young Gregory, after trying the delivery of newspapers and an apprenticeship in a boot factory, became a laboratory assistant at Bristol's new public school, Clifton College. The science master was impressed by him and helped him. From there he went on to what is now Imperial College in London.

I do not want now to dwell in detail on his career. What I am concerned with is that he became the assistant editor of *Nature* in 1893 and the editor in 1919. It was he who made *Nature* the leading world journal of science.

He wrote a number of books, one of which was called *Discovery*, and this title was taken for the journal you all know today. So the two leading science journals, *Nature* for original communications and *Discovery* for popular presentations, both owe something to Sir Richard Gregory. He was knighted in 1919. He was made a Fellow of the Royal Society in 1933.

Above all, he was the staunch supporter of the British Association and did not miss a meeting in Britain from 1883 until the year before he died. He was president from 1939 to 1946.

It is fitting, therefore, at this meeting to pay this too short tribute and add a native Bristolian to our list of West-Country scientists of the past.

MAN'S FREEDOM AND DEPENDENCE[1]

by

THE RIGHT REVEREND F. A. COCKIN, D.D.
BISHOP OF BRISTOL

Ye shall be as gods, knowing good and evil.
Genesis, iii. 5.

A RECENT issue of *Time and Tide* contained what I should judge was a fairly full summary of an article written by a member of the United States Atomic Energy Commission. Under the intriguing title 'Can we survive Technology?' the writer gave a preview of some of the lines of advance along which technological research is likely to proceed in the next few decades.

The one which struck me most forcibly was the suggestion that it is by no means beyond the bounds of possibility to devise means of raising or lowering the temperature of the earth's surface to a degree which would produce tropical or sub-tropical conditions over large areas, or, alternatively, introduce a new Ice Age.

And alongside a suggestion of that magnitude we have the recent discussions of the atomic scientists in Geneva with their forecasts of the possible effects of continued atomic releases upon the genetic structure of the human race, and the animal creation.

I need not say that I am not so foolish or presumptuous as to imagine that I can more than very partially understand, still more pass critical judgment on, matters which, even to those best qualified to speak on them, would appear to involve a number of points which are still somewhat obscure. And perhaps I may add, as a Churchman, that we have learnt some of the lessons of the nineteenth century with regard to hasty and ill-considered pronouncements as to the judgment of orthodoxy on scientific discoveries.

But no one could read the reflections of this American scientist on the possible effects of the experiments which he reckoned to be within man's compass, still less the warnings of some of those who spoke at the Geneva Conference, without realising that these are issues which must surely engage, and indeed challenge, the thinking of what Bishop Henson would have called 'considering men,' however unversed in the technical aspects of the subject. In face of the almost incalculable power which our existing, and potential, knowledge puts in the hands of those who possess it, still more, perhaps, in the hands of those who may be in a position to direct, or even coerce those who possess it, it may be allowable for those who try to speak in the name of the Christian conscience to ask men to reflect on what is implied by the concluding comment of that article, 'What *could* be done is of course no index of what *should* be done.'

There are, of course, those who would repudiate—so far as scientists themselves are concerned—the validity of the implied obligation, 'What should be done,' they would say, is no concern of ours. Others must see to that. Our business is simply to pursue our researches. But there are, as I should judge from recent evidence both in this country and in America, many who cannot accept that convenient escape from responsibility, and who are deeply concerned in conscience over the difficult and delicate question as to whether and how far the pursuit of knowledge, research, discovery, invention, admit of a controlling or even restraining moral sanction.

The ancient myth of Creation and the Fall would suggest a very definite answer. You may eat of the fruit of the trees in the garden—all but one. Thus far and no further: there is a definite limit to the range of man's freedom. I need hardly say that intelligent Christians are not

[1] The sermon preached in Bristol Cathedral on Sunday, September 4, 1955, on the occasion of the meeting of the British Association.

required today to accept that story, as literal history. We know now the nature and value of ' myth.'

And even with that allowance I doubt whether the fuller understanding of the nature of God which has come with the Christian revelation leaves us very happy with this idea of an arbitrary ban upon the use of man's powers of intelligence. ' Prohibition of technology,' writes Professor von Neumann, ' the invention and development which are hardly separable from the underlying scientific enquiry, is contrary to the whole ethos of the technological age.' I would put it on other, and I think higher, grounds. It is irreconcilable with belief in a God who is the author of man's whole mental endowment, whom we are bidden to love with all our mind.

But we need to beware lest in rejecting the face-value impression of the old story we miss a deeper meaning. There is a danger, in Reinhold Niebuhr's pungent phrase, of ' merely exchanging a wooden-headed literalism for a shallow-pated rationalism.'

Behind the bald statement of a direct prohibition there lies, I fancy, a deeper and more subtle suggestion of what one might call ' a limiting factor.' This freedom, which is man's greatest privilege, is given on condition.

His life, as depicted in what, on any enlightened reading, is seen as a high level of religious reflection, is existence between two poles—freedom and dependence. And both are to be respected if he is to maintain his true status, ' made in the image of God—but a created being,' in fact, man and not God.

For myself I am convinced that the recognition of what von Hügel used to call ' creatureliness ' is an essential condition of man's proper use of his immense and incalculable powers.

That recognition cannot, indeed, be used in support of the idea of a definite ban or limitation which can be invoked at a certain point—at any definite point. It is impossible for religion to call a halt, to deny the right of the enquiring mind to pursue its research further along this or that line, to put up warning notices ' God's private property—trespassers will be prosecuted.' But it *is* the business of religion constantly to recall man to the remembrance of his true status, and to ask him whether the *temper* in which he is pursuing his research is consistent with, proper to, that status.

Can we go further and suggest even in outline points at which this reminder may rightly become effective ? I would venture one or two such suggestions.

It may well ask man to consider the *proportion* which is observed between the many lines of possible research and application which are open. What standard, what sense of values, is controlling the aims towards the achievement of which these energies are being directed ? Into what channels are the brains and the time and the money being poured ? And what influence directs that choice, self-preservation, national prosperity, dominant commercial interests, or a comprehensive and realistic estimate of human need ? We need to be constantly on our guard against the danger of allowing some sectional or even selfish interest to take the place of dedication to the service of men, of *all* men, as equal sharers in God's bounty. And there is no point, as history (the history, for instance, of colonial expansion), and honest self-examination alike show, at which self-deception as to our motives is easier.

Deeper than this there lies the question of motive, of the spirit in which we undertake our work. I say ' we ' and ' our,' for this does not apply only to scientists. There is no calling or profession which does not need constantly to be searching its conscience in this matter. Indeed there are lessons of integrity, of readiness for self-criticism, of the repudiation of second-rate and shoddy work, of willingness to submit results to ruthless testing, which most of the rest of us could well learn from you. But it may be true, perhaps, that just because of its spectacular achievements, its capacity to turn out ' miracles ' to order, science is specially exposed to this danger of forgetting something which man forgets only at his peril.

That ancient myth describes the root of man's tragedy as rebellion, the sin of pride which will not accept the limitation of any divine sanction or authority, which seeks to become as God, knowing good and evil —determining for itself what is good and

what is evil—taking the law into its own hands and making its own morality.

That is a peril which has continually beset humanity, human nature as such. But it would seem that there are epochs and situations in which we are specially susceptible to it. It may be that by seeking to exorcise that temper from our technological age we shall be taking the first and most essential step on the road to the recovery of sanity and order in our civilisation.

THE ACT OF COMMEMORATION AND THANKSGIVING

Then shall the Dean say:

Let us now praise famous men.

Congregation : And our fathers that begat us.

Dean : The Lord hath wrought great glory by them ;

Congregation : By His great power from the beginning.

Dean : Ye shall remember those who, in all times and in all places, have devoted their gifts of intellect and imagination to interpret the laws of thought, the place of man in the universe, and the nature of reality.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all who have explored the properties of numbers, and the mysteries of time and space :

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all whose patient research has revealed the ever-changing uniformities of the forces of nature ; and made light and sound, heat and cold, wind and flood subservient to the purposes of man.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all who have distinguished the natural elements, verified their properties and affinities, and thereby compounded substances unknown before, serviceable to health and art.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all who have set in order the lineage of all living things, observed their habits and haunts, and recorded their struggles and survivals through the long lapse of time.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all who have applied the principles of the several sciences to the tillage of the soil ; the avoidance of famine and pestilence, the breeding and sustenance of flocks and herds, and the abundant provision of our daily bread.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all who by their study of the functions of life have made plain the mysteries of many diseases and abated their ravages ; all who have dedicated their lives to the alleviation of pain and suffering.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean: Ye shall remember all whose study of the diverse races of mankind, the manner of their lives, the ordering of their societies, their customs and beliefs, have furthered understanding among people and peace on earth among men of good will.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Ye shall remember all by whose vision and generosity, schools, colleges and universities have been founded, so that true religion and sound learning may ever flourish and abound ; all teachers of those who learn ; and all guides and masters of those who teach.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : And lastly, ye shall remember all who in the wise use of their abundance or their power, by their gifts of tongue and pen, by their particular skill, industry and patience, have contributed to the advancement of learning and the application of science to the common good.

Bless ye the Lord :

Congregation : Praise Him and magnify Him for ever.

Dean : Great, O Lord, is Thy Kingdom, Thy Power and Thy Glory ; great are Thy works, Thy wonders and Thy praises ; great also is Thy wisdom, Thy goodness, Thy justice, Thy mercy ; and for all these we do bless Thee, and will magnify Thy Holy Name for ever and ever.

Congregation : Amen.

ASSEMBLY OF CORRESPONDING SOCIETIES

BRISTOL, 1955

THE Assembly opened its session on Thursday, September 1, in the University Physics Lecture Theatre, when Mr. P. Ritchie Calder, C.B.E., delivered his Presidential Address, entitled *The Fragmentation of Science*, before a large and representative gathering (*see Advanc. Sci.*, XII, No. 47, 328–338). The Address was followed by a lively discussion.

Subsequent meetings were held in the University Department of Drama. The morning of Friday, September 2, was devoted to four illustrated papers by boys and girls still at school, drawn from higher forms. R. S. Cormack (Bristol Grammar School) told of his research concerning *The head plumage of house sparrows*. Published records, he said, were incomplete, since they neglected to mention a whitish eye-spot and line over the lores in the male bird. By these it was possible to sex even juvenile birds. Felicity Scott (Westonbirt School, Tetbury, Glos.) spoke on *Freshwater biology*, and dealt with a study made in a stream near Bristol. *The Charterhouse nest-box scheme* was described by J. J. Swift (Charterhouse). Tits and nuthatches had been studied in relation to size of clutch and food of young. Margaret Silcocks (Portway Secondary Modern School) presented interesting observations on *Owl pellets*.

In the afternoon members took part in an excursion to Wookey Hole, near Wells, reaching their objective via Cheddar Gorge. The succession of caves were explored under ideal conditions of floodlighting.

Bath was the choice for an afternoon excursion on Sunday, September 4, when visits were made to the Royal Pump Room, and to the extensive, though as yet incomplete excavations beneath, with their exposed wealth of associated Roman remains. Following a visit to the attached museum, members were entertained to tea by the kindness of the Mayor of Bath's Hospitality Committee. Bath Abbey was visited and a conducted tour of the city undertaken.

On Monday, September 5, Dr. F. G. Hannell (Lecturer in Geography, Bristol University) spoke on the aims and objects of a party of schoolboys, under the title of *The British Schools Exploring Society's* 1951 *expedition to central Iceland*. He presented an account of the scientific projects and exploratory marches undertaken, which was fully illustrated by colour slides and film. He was followed by Dr. H. E. Lewis and Mr. P. F. Taylor on *Photography in the Arctic*, when the many difficulties experienced in obtaining a large series of 'stills' and film, through the vagaries of weather and light conditions in Greenland were described. Ample illustrations were shown and much information was presented on the means of overcoming both physical and instrumental hazards.

A well-attended Section luncheon, held in the Berkeley Restaurant, completed the morning's programme.

The afternoon was devoted to an informative illustrated paper on *The West Country's historical contribution to science*, by Mr. C. L. Boltz (Scientific Adviser, B.B.C. European Service). Mr. Boltz traced the histories of foremost practitioners of the area, from the time of Roger Bacon in 1314 up to the year 1830 (*see* pp. 464 to 474 above).

A paper on *The city amateur, his place and prospects*, was presented on the morning of Tuesday, September 6, by Dr. W. E. Swinton (Principal Scientific Officer, British Museum, Natural History). He outlined ways in which the geology and biology of a town could be investigated by those with few opportunities for movement and described the ancillary services available to them. He also dealt with difficulties concerning apparatus and suggested that a central loan scheme should be inaugurated between the Assembly,

the Unions of Scientific Societies and the Council for Field Studies.

Professor A. Messervy, of the University of Bristol, then delivered an address on *Veterinary education in the University of Bristol*, he having kindly taken the place of the late Professor F. Blakemore, whose recent death all deplored. In tracing the progress of the student in his or her studies, Professor Messervy emphasised the importance of veterinary science to agricultural needs and in turn to those of man. In the afternoon members visited the University Veterinary Station at Langford, set in a picturesque area of Somerset, some 14 miles from Bristol, there to see for themselves the important work undertaken to fit the student for an active career.

THE ADVANCEMENT OF SCIENCE

By resolution of the Council of the Association, the June 1956 number of *The Advancement of Science* (No. 49) will form part of Volume XII, which will consist of five parts, Nos. 45–49, with a volume index at the end of No. 49.

Thereafter each volume will have four quarterly parts beginning with that for September containing presidential addresses delivered at the Association's Annual Meeting.

The price is unchanged at 7s. 6d. per part (by post, 8s.) or, by subscription in advance, 25s. (including postage) for four consecutive quarterly parts, beginning at any part.

ADDENDUM

In the article on "Genetics and Plant Breeding," *Advanc. Sci.*, **XII**, No. 47, p. 291, insert after 'crops' (line 15 of the introductory paragraph): Dr. Gordon Haskell, Department of Plant Breeding, John Innes Horticultural Institution, presented a paper on 'Genetical problems in the improvement of *Rubus* species.'

RESEARCH FILMS[1]

by

DR. A. R. MICHAELIS

PROGRESS in scientific research depends on precision instruments, because accurate quantitative data alone can form the ultimate basis of new discoveries. Although infinitely versatile, our human sense organs are very limited when accurate measurements are needed, be they of size, or length, or of weight. The invention of the microscope, the micrometer and of the chemical balance have each opened large fields of research, and their accurate measurements have greatly advanced biology, engineering and chemistry. Without such instruments, and the ciné camera is one of them, scientific research would scarcely have progressed beyond the speculative and descriptive stage. To demonstrate the latest results achieved by scientific cinematography, the Scientific and Technical Group of the Royal Photographic Society arranged a special morning session on September 3 at the British Association meeting in Bristol. In spite of many other attractions, it was extremely well attended, and the audience numbered more than three hundred.

The meeting was opened by R. J. Hercock, chairman of the Scientific and Technical Group, who introduced the programme, and by Prof. N. F. Mott, who took the chair during the morning. Prof. Mott was of the opinion that research films are well suited to bring together scientific workers from many different disciplines, and that this is one of the main functions of the British Association. He was followed by Dr. A. R. Michaelis, who had been responsible for the organisation of this session. Dr. Michaelis gave a brief outline of the history of the subject, of E. J. Marey's classical contribution and his insistence on the quantitative nature of all scientific cinematography, which he hoped would be stressed by all the other speakers. He mentioned the many cinematographic techniques which have been used in scientific research and gave a few classical examples of their applications in different fields.

The use of high-speed cinematography in physical and engineering research was the subject of W. D. Chesterman's (Royal Naval Scientific Service) paper and research film. A wide range of problems was shown on the screen, particularly noteworthy being underwater sequences of cavitation phenomena, partly recorded on models in hydrodynamic flow channels and partly recorded in the open sea. Spark cinematography at frequencies up to 100,000 frames/sec. was demonstrated in ballistic research work and showed in detail the impact of projectiles on armour plating. Mr. Chesterman concluded with a plea for the wider use of underwater cinematography by marine biologists, who might well use this established technique for the study and recording of their subjects in natural surroundings.

An example of the use of high-speed cinematography in industrial research was presented by D. J. Dearnley (Central Research Establishment I, National Coal Board). Froth flotation of fine-coal particles was cinematographically recorded at 2,500 frames/sec. in order to elucidate the mechanism and efficiency of grain attachment to air bubbles. A field-splitting optical system was developed in order to record each bubble simultaneously from two directions at right angles, and from the resulting research film, received with applause at the meeting, it proved possible to measure collection efficiency and contact times as well as to plot the trajectories of particles and bubbles.

The most outstanding research film shown at the meeting was the one dealing with the behaviour of living cells in tissue culture, presented by E. J. Ambrose (Chester Beatty Research Institute, London) and recorded in collaboration with M. Abercrombie (University College,

[1] The Scientific Films Committee of the British Association arranged daily programmes of scientific films at the Meeting in Bristol, August 31–September 7, 1955. A special session devoted to research films was held on the morning of September 3.

London). Combining the new technique of Smith (Baker) interference microscopy with time-lapse cinemicrography, Dr. Ambrose was able to show a colour film which left a deep impression on all who saw it and which was received with much applause. The advantage of this new technique was the demonstration of changes in thickness of cells, shown by marked colour changes, and thus providing an extra dimension in the depth of the recorded image. In the film, the behaviour of normal and cancer cells was contrasted, showing that, when two normal cells make contact, adhesions occur on their surfaces which do not take place in the case of tumour cells. Particularly outstanding was the record of the behaviour of cancer cells in the presence of ribonuclease ; this enzyme stimulated the free movements of the cancer cells even further and led to their death through over-digestion of the enzyme.

High-speed cinematography through the microscope, a rare and extremely difficult research technique, was demonstrated with three examples by R. McV. Weston (Simpl, Ltd.). After explaining the technical problems of the technique, Mr. Weston showed its use for recording the movement of mercury in a clinical thermometer, for examining the lever escapement of a watch and for the discharge of elaters and spores from a liverwort. The first two films showed hitherto unknown defects, and the third allowed quantitative evaluation by Prof. C. T. Ingold (Birkbeck College, London), for whom it was taken. In the absence of Dr. J. S. Courtney-Pratt in the United States, Mr. D. P. C. Thackeray (Research Laboratory on the Physics and Chemistry of Surfaces, Cambridge) presented a brief paper and a research film made by the Courtney-Pratt image-sampling technique ; the subject of the film was the explosion of single crystals.

X-ray cinematography, both of the direct and the indirect method, was shown by G. E. H. Foxon (Biology Department, Guy's Hospital Medical School, London) ; the ingenious use of ' Meccano ' gears was greatly admired in a film more devoted perhaps to an exposition of technique than of results. J. Underwood (G.K.N. Group Research Laboratory) gave the last paper, describing further examples of the use of high-speed cinematography in industrial research : his first high-speed film showed the passage of liquid in a heat-exchanger model ; his second the initiation of a welding arc under an argon shield by a condenser discharge ; and his third, taken at 5,000 frames/sec., allowed the determination of the acceleration characteristics of the blades of a flying shear. Altogether, thirteen films were shown at the meeting, which concluded with excerpts from the American research film, ' Color Motion Pictures of the Whole Sky '.

Four German research films which had been specially sent over for this meeting were projected two days later during the general screening of scientific films. Two of these were ' Netzbau der Kreuzspinne ' and ' Pervitinwirkung bei der Kreuzspinne ' ; both were made by Prof. H. M. Peters (Zoological Institute, University of Tübingen). The second of these two was particularly noteworthy, as it allowed the quantitative evaluation of the action of benzedrine on the spider *Aranea diadema* ; this was achieved by plotting against time the intervals along the radial threads of the web. ' Biologie des Hamsters ', Parts 1 and 2, were produced by Dr. I. Eibl-Eibesfeldt (Department of Animal Behaviour, Max-Planck Institute for Marine Biology, Buldern, Westphalia) and showed some remarkable instances of mating behaviour of hamsters as well as of their young. These four films were lent by the Institut für den wissenschaftlichen Film, Göttingen, through the courtesy of its director, Dr. G. Wolf.

There can be no doubt that this first collective presentation of research films from many disciplines at a meeting of the British Association was a great success, as the frequent and often lengthy applause by the audience amply demonstrated. A similar session is being planned for the forthcoming meeting of the Association in Sheffield (August 29–September 5, 1956) and it is hoped that all those who have made research films in the meantime will use the opportunity to present their results ; the films should be submitted to either the Scientific and Technical Group of the Royal Photographic Society or to the Scientific Film Committee of the British Association.

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